REPORT

ON

THE PREPARATIONS FOR, AND OBSERVATIONS OF,

THE

TRANSIT OF VENUS,

AS SEEN AT ROORKEE AND LAHORE,

ON DECEMBER 8, 1874.

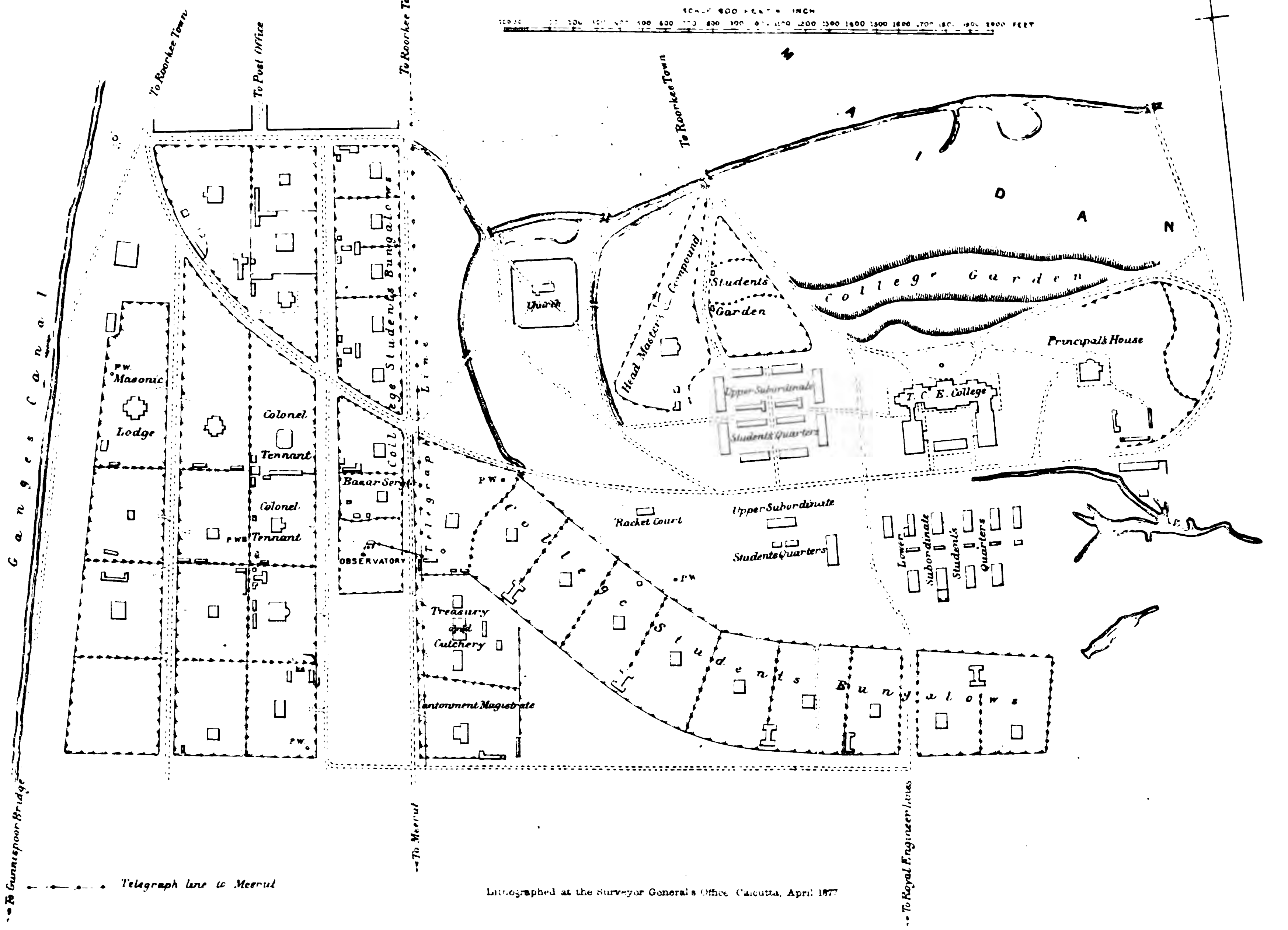
BY

COLONEL J. F. TENNANT, R.E.,

F.R.S., F.R.A.S., & F.M.S.

CALCUTTA:
OFFICE OF THE SUPERINTENDENT OF GOVERNMENT PRINTING.
1877.

SKETCH OF
PART OF
ROORKEE.



Lithographed at the Surveyor General's Office, Calcutta, April 1877

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PREFACE.

THE printing of these Reports has been delayed by various causes mostly beyond my control, among which was my transference to Calcutta before it was revised.

It will be seen that I have departed in one point from Sir George Airy's instructions. I have left all the equations with seconds of space as a unit, instead of taking the last step, and making the unit a second of observed time. In all the calculations connected with the observations near contact, I have felt that except in the contacts themselves, the probable error of observed time was of no importance, and that that of space alone affected the result; in the case of the Alt-Azimuth Observations of course it was the errors in space which were sought. Though I had at one time intended also exhibiting these results here as proposed by Sir G. Airy, I have finally left them, for uniformity, as they stand in my papers.

The original memoranda of Observations, and one copy of the Computations I shall, as early as possible, transmit to the Royal Observatory, the other copy of Calculations I propose to retain.

J. F. TENNANT, *Col., R.E.*

H. M.'s Mint, Calcutta,
May 21, 1877.

REPORT

ON

THE TRANSIT OF VENUS.

ON DECEMBER 8, 1874.

ERRATUM.

The reader is requested to correct the following errors which have been overlooked:—

Page 2, line 5 from bottom *for* "of" *read* "and"

" 11, line 5, *for* "flexture" *read* "flexure"

" 27, line 11, *for* "and find" *read* "which give"

... private instruments, which were afterwards to be transferred to a Solar Observatory.

A copy of the British Association's resolution having come into my hands, I was able to join their authority to that of the ASTRONOMER ROYAL in addressing the Secretary of State, which I did on September 12. The result was a reference to India, and after a long time it was determined to procure the instruments for the Transit of Venus and Solar Observation. The Photoheliograph alone was first ordered; but the delay had made this too late, and it was only by Dr. DE LA RUE's kindly giving up an instrument he had commissioned for his own use that it became possible to provide for photographic observation. The other special instruments were not finally ordered till the early part of 1874. Their manufacture was superintended by Colonel STRANGE, who placed them in the hands of

Messrs. COOKE of York. Probably instruments in hand were completed for the occasion; but the short period available for preparation makes the fact that all were ultimately used in December, very creditable to those concerned, and fully accounts for the small defects, which were unavoidable under such circumstances of hurry.

When it was determined that I should be employed to superintend the observations, my first care was to select positions for the observing stations. The Punjab is often cloudy in December, and thus it was desirable not to go too far west. Then it was absolutely necessary that there should be some facilities for altering and repairing instruments which one could hardly expect to find perfect in all respects, and (not knowing what were coming) I contemplated getting what I could, and making the necessary adaptations. As there was no hope of any of the party being specially trained at Greenwich, it was considered necessary to provide that the instruments should (if received) be in position for some time, and no temporary structures could possibly have stood the burning heat of an Indian hot season, and the heavy rains which follow. Hoping, too, to use such parts of the Observatory as could be moved in the proposed establishment for Solar Physics, I was led to contemplate a much more substantial style of Observatory than that sent with the English expeditions, and one which would require some workshop facilities for its completion. I, therefore, selected Roorkee, a station to which I was called by other duty, for my main station, and I resolved, if possible, to send an officer to some station to the West to observe the last contacts of Venus with the Sun, and to use for this purpose a 6-inch telescope by Mr. SIMMS which Government had enabled me to purchase in 1871 for the Eclipse observations at Dodabetta. If the specially-provided instruments had not arrived, I should, by giving up the western expedition, still have been in a position to make some observations at Roorkee. These arrangements having been approved by Government, I waited for information as to the forms of the new instruments before proceeding to design cover for them, but at last (without this) I was obliged, in November 1873, to submit plans of estimates for the proposed Observatory, which were sanctioned. I did not, however, begin building (though the revolving domes were ordered) till February 1874, still waiting for such details as might guide me. Shortly after I had begun, I heard that the instruments had at last been ordered,

but many points were still uncertain, and I was finally obliged to accommodate the clock and the Photoheliograph by placing large heavy stones on the tops of the brick pillars I had prepared, which were too small. Fortunately, with these exceptions, the arrangements proved convenient.

The general plan may be seen from the drawings. The piers for the instruments are of brick-in-mortar as good as I could procure; each is capped by a stone to carry the instrument. The walls up to the level of the plinth are of similar construction, as also those of the round rooms containing the instruments used in observing the actual Transit. The walls of the rectangular building above the plinth are of brick laid in mud and plastered outside with mortar and inside with earth, which gives the Transit room the pleasantest color for working in that I know of. The roof of this is a flat brick arch, tied at intervals with wrought-iron rods. I adopted this construction on the recommendation of the Superintendent of the local workshops, who also sent men to put it up. It is cheap, and in this case proved weather-proof, which is not its usual character. The floors are all of wood and rest entirely on the walls, leaving a space of from one-half to three quarters of an inch all round the pillars.

The construction of the rotating domes was the source of very serious delay. The middle of September came before even one was thoroughly out of hand in a barely workable condition, and it was only on the 1st October that all three were ready. The domes consist of wooden frames on cast-iron curbs covered with zinc and lined with wadded cloth to keep them fairly cool; they are each turned by a handle, which causes a pinion to act on teeth cast in the upper curb. They have given a great deal of trouble from the hurried way in which they were finished, but I believe would be quite efficient if taken down and properly put up.

I have now to speak of my assistants. Captain Campbell, R.E., of the Great Trigonometrical Survey, was placed at my disposal by Colonel Walker, the Superintendent: he had to go to England for a short time in the end of 1873, and advantage was taken of this to employ his services in examining and verifying the instruments: his knowledge of them thus acquired was of essential service to me when they had hurriedly to be put together, and I in every way benefited by his skill and

experience. Early in 1874, I had borrowed the services of three men of Her Majesty's 55th Regiment, whom I purposed instructing in Photography, and I had intended exercising the general superintendence of this department myself, believing that all work having been reduced to a routine, and the instruments having been tried and put in thorough order, I should have no difficulty in arranging any small assistance. The great delays of which I have spoken, however, made the success of this plan doubtful, as it gradually became evident that, instead of each instrument successively being adjusted and studied, all would have to be taken in hand at once. I was, therefore, glad to learn that my old friend, Captain WATERHOUSE, would, with the permission of Colonel THUILLIER, R.A., (the Surveyor General) be able to join me; and one of my trained Photographers wishing to leave me at the last moment, I was glad to borrow one of the men from Captain WATERHOUSE's office. The Roorkee party then stood as follows:—

Captain CAMPBELL, R.E.
,, WATERHOUSE.
Sergeant HARROLD, R.E.
Lance Corporal GEORGE, } H. M.'s 55th Regiment.
Private FOX, }

and I had a small establishment of natives to aid in the various duties.

The work was divided as follows:—

Time determination	Colonel TENNANT.
Equatorial observations	,, ,,
Alt-Azimuth	Captain CAMPBELL.
Photoheliograph	,, WATERHOUSE.

A day or two before the Transit, Captain HEAVISIDE, R.E., came to Roorkee and kindly undertook to look after the Chronograph during the observations, winding up the used records strips and putting in new ones if necessary: he was also to observe the last contacts with the Royal Society's telescope, by SLATER, which he had brought with him.

My western station at Lahore was taken by Captain GEORGE STRAHAN, R.E., also by permission of Colonel THUILLIER; he was sole observer.

I shall in this Report deal with the Roorkee observations as made by myself and Captain CAMPBELL, and those made by Captain STRAHAN at

Lahore, and I shall add a statement of the results deduced from Mr. Hennessy's observations at Mussooree, which I have reduced for comparison, though they were in no way under my superintendence.

The whole of the photographs with all data for their reduction have been transmitted to the Astronomer Royal, to be dealt with like those from other parties; and I have furnished all necessary information for reducing them. I must leave the discussion of their results for the Astronomer Royal, and have touched on this work solely to place on record suggestions for future work.

DESCRIPTION AND POSITION OF ROORKEE.

Roorkee is a small station in the North-Western Provinces, about 20 miles easterly from the Railway Station at Saharanpore. It is near the eastern edge of the high land included between the beds of the Ganges and Jumna: immediately to the north-east is the bed of the Solani, a torrent that issues from the Sewalik range and terminates in the Ganges, and over which the Ganges canal is passed by a large aqueduct to which Roorkee owes its existence, as, during the construction it became the head-quarters of the Canal Department in the North-Western Provinces, the site of extensive workshops, and of a College of Civil Engineering: it has since been enlarged by becoming a small military station. Over the bed of the Solani from N.N.W., nearly to the east, the Sewalik range and the Himmaleh are seen in clear weather; some of the great snow peaks rising high into the air. The geodesical position of Roorkee, therefore, is not likely to agree with the astronomical determination.

In the cold season of 1873-74, Colonel Walker, R.E., Superintendent of the Trigonometrical Survey, detached an officer of his department for the purpose of connecting some points in Roorkee with the principal triangulation of the survey that through them I might connect the observatory with it. I found that connection by triangulation was impossible,

owing to trees; but I made a traverse from Roorkee Great Trigonometrical Survey Station, past the site of the Observatory and terminating in the College dome. By this I find that the centre of my Transit pillar is 4524·3 feet distant from Roorkee Station at an azimuth of 181° 13′ 58″: whence its position by the elements of the Great Trigonometrical Survey are (when reduced to Madras taken at its present accepted value as an origin of longitude)—

N. Lat.	...	29° 51′ 50″·95.
E. Long.	...	77° 52′ 49″·1 = 5*h*. 11*m*. 31*s*·3.

The Photoheliograph and Alt-Azimuth pillars are each 19 feet south of the Transit pillar, and the Equatoreal 53 feet South. I have deduced from these data for the geocentric latitude of the equatoreal 29° 41′ 57″·9, and have thought it sufficient to retain the same value for the Alt-Azimuth.

In order to reduce the Alt-Azimuth observations, it is necessary to have the astronomical latitude. This was accordingly determined by Captain Campbell with the 36-inch theodolite by circum-meridian altitudes of Nautical Almanac Stars with the following results:—

By North Stars.				*By South Stars.*			
γ Ursæ Majoris,	...	29° 51′ 33″·63	8 obs.	α Hydræ	...	29° 51′ 33″·73	4 obs.
δ „ „	...	34·31	6 „	α Leonis	...	35·08	10 „
α „ „	...	33·92	6 „	ρ „	...	32·35	4 „
				δ Crateris	...	33·50	6 „
Mean by North Stars		29° 51′ 33″·95		Mean by South Stars		29° 51′ 33″·67	

General Mean 29° 51′ 33″·81 N.

I have used 29° 51′ 33″·8 for the reduction.

Of course, the longitude is also affected by the attraction of the mountain ranges. Provisionally I assume it in the calculations as being 5*h*. : 11*m*. : 31·00*s*. east, but I have been enabled, by the co-operation of the Telegraph Department and Mr. Pogson, Director of the Madras Observatory, to determine directly, with sufficient accuracy for my purpose, the difference of longitude between Roorkee and Madras. A line of

wires having been laid by the Field Telegraph Department of the Sappers from the Observatory to the Telegraph Office in the station, was there joined on to the main line, so that signals could be sent directly from the Observatory to Agra where they were translated on to Bombay, and then again translated to Madras. The best plan would, of course, have been to have recorded the signals at each end on a chronograph also used for transits, but I found it impossible to make these arrangements and the following was the procedure. The signals at Roorkee were made by tapping an ordinary key for every 10th second of the sidereal clock for three minutes, and received by Mr. Pogson at Madras (who preferred his ear to a chronograph), and who used his mean time clock of which he considered the error and rate to be well known. The signals at Madras were made in the same way, but I arranged that the same pricker of my chronograph which was used in transits should be worked by its own battery when the current from Agra passed through a relay in the Observatory; they were thus automatically recorded. Two exchanges of signals were made on each night, but some were missed from various causes, though very few. The results are as follows:—

On May 31, 1875, I received from Madras in all 37 signals, one being missed, and Mr. Pogson also received 37 signals, one being lost. On June 2, I received 35 signals and Mr. Pogson 38, and on June 5, we each received the full number of 38.

The deduced longitude is: Roorkee West of Madras	May 31, 1875 ...	9*m*. : 26·18*s*.
	June 2, „ ...	26·02*s*.
	„ 5, „ ...	26·33*s*.
Mean, Roorkee West of Madras		9*m*. : 26·18*s*.

The transits at Madras having been taken by eye and ear, all have been reduced by connecting the Observer's equations in the following way through Captain Campbell when he passed through Madras.

It was found by experiments at Roorkee on two nights that Captain Campbell observed transits earlier than I by 0·08*s*., each using the chronograph: we observed alternate Nautical Almanac Stars, but on the second night he observed mine of the first night, and I his. Then on

a later night he observed a good many unknown stars,* both by eye and ear and by chronograph, whence it was found that his chronograph transit was later than that by eye and ear by 0·04*s*., so that my chronograph transit is 0·04*s*. later than his transit by eye and ear. I have not the data of the Madras comparison. A small correction + 0·04*s*. has been made to the times of Roorkee signals for the apparent retardation in record (found by experiment) of signals made in the same way as those sent.

The longitude of Madras used is sensibly the same as that given in Volume XVI., Royal Astronomical Society's Memoirs, by the late Mr. TAYLOR, and indeed, I believe, no attempt has been made to determine it since that time, 30 years ago. We have then—

Longitude of Madras	5*h*. : 20*m*. : 57·27*s*. East of Greenwich.
Roorkee, West of Madras ...	9*m*. : 26·18*s*.
Longitude of Roorkee... ...	5*h*. : 11*m*. : 31·09*s*. East.

which value is probably as good as can be given till chronographic determinations are resorted to throughout.

The time required for signals to pass on the three nights was 0·24*s*., 0·24*s*., and 0·22*s*, the distance being 1,931 miles with two translations.

The second set (one-half) of signals from Roorkee was received at Madras on each night by chronograph, as well as by ear, but the times by chronograph differ systematically from those noted by Mr. POGSON by ear, and as the transits were all taken by eye and ear I have preferred adhering to Mr. POGSON's noted signals, thinking it more probable that he referred signals and stars to the clock beat alike, than that the chronograph recorded the signals in the same way that Mr. POGSON would have noted transits.

When the longitude of some point in India has been determined by the modern methods, it may possibly be worth while to ascertain again the relative position of Roorkee, but it is hardly worth going further till this work, most important in a scientific point of view, is carried out.

* The system of wires afterwards described is not what was used for this purpose. Finding the intervals too great for convenient use, I had a new set of wires placed in the instrument, whose distances from the middle one were approximately 32*s*., 16*s*., 8*s*., and 4*s*., on each side. The four outer wires were observed by the eye and ear method, and the five inner ones with the chronograph.

Indian longitude must eventually furnish the data for the Australian Colonies and the large islands near the Equator, whence through China we may hope to complete the whole circuit of the world and establish points of reference everywhere. There is, of course, an alternative circuit through Siberia, if the mere feat of surrounding the world were alone in question, but India is an essential stepping stone to the British, Dutch and Spanish Colonies, and I think that no time should be lost in establishing, beyond all doubt, the longitude of some point in the country with all the accuracy attainable by modern appliances.

The height of my station was determined by connection with the bench marks of the Ganges Canal which are connected with the lines of level of the Great Trigonometrical Survey, and by them to the sea level at Karachi. The floor of the Observatory has thus been found to be 876·2 feet above the sea. The effect of the mountain attraction, however, would be to make this too small as an increment to the earth's radius Vector. I have endeavored to allow for this by assuming the increment to be 950 feet and the logarithm of ρ $\left(\frac{\text{radius Vector}}{\text{equatoreal semi-diameter}}\right)$ becomes 9·9996618, of which five figures would be unaffected by any possible difference in estimating the mountain masses.

ROORKEE STATION.

Determination of Local Time.

My first care was to secure the means of determining this essential. Having occasion to go to Calcutta in December 1873, I examined the stock of the Government there, and selected a Portable Transit of 30 inches focus which seemed little used, and directed that it should be put in order for me. It reached me a couple of months later, and as soon as I had any cover for it I lost no time in testing it. To my disappointment the results were hopelessly wild. It was only after weeks of trouble during

the hottest weather in experiment and rectification of defects that I got it to work at all; then, with great caution in touching it, it would give fairly accordant results, though the position was in a constant state of change with temperature. The transit made for the purpose of these observations only reached me on November 24, and it was immediately put in position. It was impossible, however, to use it with any satisfaction till various changes were made, and at that late period I was very unwilling to undertake these, but a natural reluctance to revert to an unstable instrument and the advantages [urged by Captain CAMPBELL] of reflection observation of the wires if I could get them visible, induced me to enter on this work; the necessary alterations in the illumination of the wires and adaptation of the eyepieces were made, and it was hoped that the difficulty of seeing the wires by reflection was got over.

This instrument (by Messrs. COOKE and SONS) is, I understand, a combined design of Mr. COOKE, Colonel STRANGE, and Professor MAGNAGHI. A strong central cube carries the frusta of the cones which form the Transit axis on two of its sides, and the tube of the Telescope is fastened to other two. The object glass has an aperture of 3 inches, and a focal length of about 34¾; the eye end is furnished with two micrometers at right angles to each other; one carried the system of Transit wires, and the other a single wire to be used in conjunction with a delicate level for determining latitude by means of differences of Zenith distance. The Transit axis is 18¼ inches long, and terminates in (what are apparently) steel pivots resting in segmental bearings. Both pivots are perforated, but the light passing through one alone falls on the wires after its reflection from a small metal surface in the centre of the cube. There is only one setting circle near the eye end of the telescope which is counterpoised on the other side; its verniers read to half a minute and are connected with two levels, one of which is the fine one for use with the vertical micrometer. The pillars carrying the bearings of the Transit axis are cast in one mass with a weighty base plate. Pillars and plate turn on a second plate to which they can be firmly bolted in any position, and a slow motion for accurate relative adjustment is supplied. The lower portion is supported on 3 foot screws of Colonel STRANGE's pattern, and is furnished with a few divisions to enable the direction of the Transit axis to be changed by a right angle so as to allow Transits to be taken in

the prime vertical. There is a very convenient reversing apparatus which enables the collimation to be determined with a single collimator. The inclination of the Transit axis to the horizon is determined by viewing the wires by reflection from Mercury. Levels were supplied to be attached to the central cube for the purpose of watching the flexture of the Transit axis; I have not used these except for roughly levelling on occasion; I have no description of their adjustments; have failed to find any convenient mode of making them; and believe that they are perfectly unnecessary.

As a Transit, the performance of this instrument is excellent: I believe time can be determined with it as well as in any permanent Observatory. In its other capacities I have no experience of it; but I am disposed to think that it would be better without the arrangement for differences of Zenith distance whose micrometer is inconvenient when using a high power diagonal eyepiece; a very much better arrangement, I think, would be to have only a single micrometer which could be turned round so as to have the moveable wires either vertical or horizontal.

The clock is by Messrs. COOKE and SONS: it does not seem to have any peculiarity. It marks a chronograph by means of a wheel (on the same axis as the scape wheel) of which the teeth pass a spring just before the scape wheel is stopped by the pallets; one tooth, as usual, being cut away to leave out the mark at the even minute. Three journeymen clocks or dials were also worked by the same clock, the contacts for this purpose were made by a pin on the pendulum which, in the middle of its beat, depressed an agate stud and thus pressed two springs into contact. This has not been found satisfactory. It worked (after a great deal of trouble) for some time, but the contacts became burnt, and I have reason to believe that it affected the rate of the clock very irregularly.

All time signals from every source were recorded on a chronograph specially made by Messrs. COOKE and SONS. My plans for observing the Transit of Venus originally involved a large number of Transits, both with an alt-azimuth and equatoreal. It seemed to me impossible that writing down eye and ear observations could meet the want, and I was, therefore, desirous of having a chronograph which should record all the times of observation. As regards the Photoheliograph, anything which

should give the time of observation, even to a quarter of a second, would answer, and as regarded any of the other instruments it seemed to me that if the probable error of a reading were as small as that of an observation in the old way, there would be a gain, and looking to the large probable error of eye and ear, and even chronograph-recorded observations of Transits, it seemed that extreme accuracy of motion might be dispensed with; and that, even limited as the time for preparation would be, a sufficiently good apparatus could be made to record the observations of several instruments on different pieces of paper. Messrs. COOKE and SONS have, under Colonel STRANGE's instructions, made an instrument which, I believe, is adequate to any astronomical work, and which furnishes separate records of four instruments at a very moderate cost. The mode of governing the motion is by a clock precisely similar to that which is used by the Messrs. COOKE and SONS for their equatoreals with success: it must be evident to all, I think, that an arrangement which can keep a star bearably on a wire must be quite adequate to produce a movement equable enough for ordinary chronographic work, and I believe it does so. The bevel wheel on the clock, which usually drives the shaft leading to the tangent screw of the polar axis, is here made to drive a long horizontal shaft, which again drives a transverse shaft at each end of the table. The ends of the transverse shafts have wheels fastened on them, which draw out the fillets of paper on which the record is made. Each fillet has to pass under two prickers, one for the clock, and the other for the observer. The instrument has, on the whole, answered well: the principal trouble has been from the fillets becoming displaced. This is, I believe, principally due to the size in the paper, which becomes hard in dry weather, and does not readily bend round the rollers, hence the paper occasionally slips off the guides and stops the clock. Suitable guides will prevent this, and I prevented any mischief from this cause for a long time, though unfortunately I did not know of the defect when the chronograph was most wanted. There is, of course, a correction, differing for each record, to reduce the mark of the observer's pricker to that clock, but it is very small, and I have not thought the refinement of using it necessary.

The whole of the Transits for the determination of time were made by me. The places of the stars employed were mostly taken from the

Nautical Almanac for 1874, but I have used some others, of which I annex a list, with their adopted places for 1874·0, and the authorities for the places—

Stars' names.	Mean Æ. 1874·0.			Authority.
	h.	m.	s.	
Bradley, 3147	23	27	50·20	Greenwich 7 y. Cat. for 1864.
„ 1446	10	24	19·48	Ditto and Results for 1868, 1869, and 1870.
Groombridge, 2053 ...	13	42	08·67	Greenwich 7 y. Cat. for 1864.
„ 3834 ...	22	30	03·38	Ditto.
ε Eridani	3	26	59·68	Nautical Almanac, 1875.
17. Tauri	3	37	23·74	Gr. 7 year, Catalogue for 1864.

The intervals of the wires were determined in terms of the micrometer screw on November 25, 1874; by making five intersections with each of the cross of a Collimator, the following results were obtained:—

Wires.	Mean Reading on Cross.	Distance from D.
	rev.	rev.
A	2·0128	13·8788
B	8·9550	6·9366
C	12·4160	3·4756
D	15·8916	......
E	19·3468	3·4552
F	22·8000	4·9084
G	29·7792	13·8876

In the position of the eye-end held up to November 29, the value of a revolution was 3·97273s. as determined by Transit of ϱ Cephei, B. A. C.

8026, 8215, 651, 896, and 879 over the wires A and G. Hence the equatorial intervals adopted to that day, inclusive, are—

A to D.	B to D.	C to D.	E to D.	F to D.	G to D.
s.	*s.*	*s.*	*s.*	*s.*	*s.*
+ 55·13673	+ 27·55724	+ 13·80762	− 13·72658	− 27·44521	− 55·17169

On November 29, I became convinced that the plane of the image of the wires seen after reflection was not that of the wires seen directly; this was corrected next day, and on December 14, I observed Transits of B. A. C. 7167, 8074, 8124, 8217, 8334, 39, 86, 194, and 280 for wire intervals. These were deduced in the usual way to be as follows:—

A to D.	B to D.	C to D.	E to D.	F to D.	G to D.
s.	*s.*	*s.*	*s.*	*s.*	*s.*
+ 55·2710	+ 27·6021	+ 13·5739	− 13·5988	− 27·4800	− 55·1016

The observations, however, being less accordant than I could have wished, I have deduced the value of a revolution of the screw by comparing the sum of these intervals with the sum of the distances by screw, whence 1 Rev. = 3·96825*s*., and I have used the intervals (calculated from micrometer intervals), which follow in preference to the last—

A to D.	B to D.	C to D.	E to D.	F to D.	G to D.
s.	*s.*	*s.*	*s.*	*s.*	*s.*
+ 55·07455	+ 27·52616	+ 13·79205	− 13·71110	− 27·41426	− 55.10947

The position of the optical axis of the telescope has been determined by taking five readings of the cross of the Collimator in each position of the instrument. From December 1, this has been done both before and after observing. Up to November 30, the inclination of the Transit axis was occasionally found to be best got by superposing the wire on its ill-defined image: after that date it has always been placed so as to touch its image alternately on each side, and the inclination observed both before and after taking the night's Transits.

The instrumental corrections have been computed as follows:—

Collimation correction for centre wire = reading of micrometer for optical axis — reading for Transits — 0·0045*s*. (for diurnal aberration.)

Level correction = reading for optical axis — reading at coincidence of direct and reflected wires.

The azimuth correction has been determined nightly by stars above and below the pole, except on November 28, when a north and south ster were used, and on November 29, when two south stars are combined with a north one, and each night's Transits was reduced by the azimuth as determined on that night. It being now evident from the Collimator readings that the changes of azimuth of the instrument after December 2 are rather the result of errors of determination than of instrumental changes, I proceeded to reduce all the clock errors to what would have been obtained on the hypothesis that the azimuth remained after this day constant at its mean value up to December 11, and the last column shows the clock error so adjusted.

The table on the following page shows the information necessary for deducing the clock's errors and the mean corrections as reduced.

It is to be understood that I mean by "clock correction" the time which is algebraically additive to the clock time, in order to get sidereal time, and the clock rate is the increase of clock correction in 24 hours. The following table shows the clock time of mean of observations, the correction and rate for each day from December 3 to December 11:—

Date.	Clock Time, Mean Observation.		Clock Correction	Rate.
	h.	*m.*	*s.*	*s.*
December 3 ...	2	18·1	— 2·01	......
„ 4 ...	2	13·8	— 2·35	— 0·34
„ 5 ...	2	34·8	— 2·78	— 0·43
„ 7 ...	2	14·3	— 3·53	— 0·38
„ 8 ...	2	13·8	— 3·87	— 0·34
„ 9 ...	2	52·4	— 3·92	— 0·05
„ 10 ...	2	52·4	— 3·94	— 0·02
„ 11 ...	2	42·8	— 4·14	— 0·20

I have assumed that on December 8 at 14*h.* 05*m.* by the clock, the correction was — 3·88*s.*, and the rate — 0·08*s.*

1874, Date.	Micrometer Readings.				Calculated Corrections.			Clock Corrections.	
	Collimator F. East.	Optical Axis.	Coincidence of direct and reflected wire.	Setting for Transits.	Level correction.	Collimation of D.	Azimuth.	Computed.	Adjusted.
	Rev.	Rev.	Rev.	Rev.	s.	s.	s.	s.	s.
Nov. 28 ...	11·2046	11·2906	11·2787	11·2860	+ 0·04728	+ 0·00040	− 0·3754	− 0·13	...
„ 29 ...	·2368	·2939	·2649	·2980	0·11521	− 0·03417	− 0·4511	− 0·33	...
„ 30 ...	·3934	·2799	·2144	·2750	0·25992	+ 0·00159	− 0·4914	− 0·81	...
Dec. 1 ...	·1298	·2947	·2008	·2900	0·37262	+ 0·00079	− 0·1988	− 1·52	...
„ 2 ...	·1260	·2634	·2167	·2350	0·18532	+ 0·09484	− 0·7545	− 1·60	...
„ 3 ...	·2828	·2802	·2119	·2650	0·27103	+ 0·04246	− 0·4599	− 2·05	− 2·01
„ 4 ...	·2848	·2762	·2275	·2420* ·2740	0·19325	+ 0·11786* − 0·00913	− 0·6108	− 2·34	− 2·35
„ 5 ...	·2810	·2794	·2090	·2700	0·27936	+ 0·01944	− 0·4205	− 2·80	− 2·78
„ 7 ...	·2816	·2785	·2050	·2800	0·29167	− 0·02381	− 0·5365	− 3·54	− 3·53
„ 8 ...	·2820	·2765	·2080	·2700	0·22421	+ 0·00774	− 0·7401	− 3·79	− 3·87
„ 9 ...	·2855	·2767	·2099	·2800	0·26508	− 0·03095	− 0·6371	− 3·90	− 3·92
„ 10 ...	·2845	·2796	·2041	·2700	0·29960	+ 0·02024	− 0·5964	− 3·93	− 3·94
„ 11 ...	·2876	·2783	·2213	·2700	0·22619	+ 0·01508	− 0·5701	− 4·14	− 4·14
„ 16 ...	·2820	·2809	·2427	·2800	0·15159	− 0·01429	− 0·6707	− 4·14	

* This only applies to one star.

THE PHOTOHELIOGRAPH.

I had intended, as I have before said, to take the work with this instrument under my own supervision. I believed that I should have had ample opportunities for experiment and for instructing assistants, as during the Transit of Venus the work would be merely of routine. I did, in fact, instruct three men of Her Majesty's 55th in all the ordinary operations of wet collodion photography, but the instrument was late, and cover for it later, nor did the special chemicals arrive early enough. I had, therefore, no means of experimenting on dry plates, and it was evident that I should be so much pressed with the arrangements generally, and that unless I abandoned other work, I should have great difficulty in arranging for the Photoheliograph. It was, therefore, very acceptable to learn that my old Assistant Captain WATERHOUSE, would be again able with the Surveyor General's permission to join me and relieve me of this instrument, and that he could bring a man who was used to photographic operations to aid him. Captain WATERHOUSE, even before joining me, entered very zealously into the question of dry plates, and made hundreds of experiments in Calcutta, where he succeeded well with them.

As soon as I had cover, I placed the instrument approximately in position, about which there was no difficulty; but when I sought to complete its adjustment, I found that I could only use the sun, as stars and planets were invisible in the Camera. After some trouble I got a small telescope fixed onto the leaden counterpoise, and I was thus able to use the stars for adjustment and get the polar axis into position with reasonable facility and sufficient accuracy. Having done this, I made the instrument over to Captain WATERHOUSE, who was quite competent for the photographic business. He has furnished me with a detailed report of his operations, a copy of which has been sent to the ASTRONOMER ROYAL, together with all the photographs, it being intended that the measurement and reductions shall be made precisely as for the British Government's expeditions.

I shall, therefore, here only speak of the instrument and such points as nearly a year's experience of working it have brought to my notice.

As to processes for Photographing.—In the first place all dry processes failed in giving satisfactory results at Roorkee. Years ago when stationed there, I had met with similar results, and had become doubtful whether it was desirable to adopt such uncertain procedures, but I hoped that the difficulty would yield to systematic experiment. It is evident, however, that want of knowledge cannot account for Captain WATERHOUSE's difficulties with processes he had proved in Calcutta, and the only solution which occurred to us was that particles of lime from the walls must be floating about in the dust inevitable in a dry climate. The great difficulty we had before experienced with Mr. DE LA RUE's procedure with a simply iodized collodion made us both feel more confidence in bromoiodized collodion developed with an iron and sugar solution which (as we found in 1871), gives nearly as fine a deposit as pyro-gallic acid. I have since found that this was unfortunately the worst developer we could have used. The fact is that there is a superabundance of light and a developer whose great feature is the bringing out of slight impressions of light without solarizing the brighter parts is just what is not wanted. I have succeeded best with a pyro-gallic solution greatly restrained by citric acid, and next to this with iron similarly restrained. The best results have been with some collodion made after a formula I used in preparing for Captain WATERHOUSE, and some are quite equal to the best dry plates;* but I have used various mixtures of Thomas' collodions with fair success, though I am disposed to think his collodion for iron development, as good as any of these if used alone.

The instrument seems to me to vibrate too readily, but its weight would probably be a good deal increased if this were rectified entirely. The great defect appears to be want of flatness of field: greatly as the enlarging lens has been improved, it seems to me to share the defect in this respect of all photographic lenses. It is possible to get fair definition over the central part of the sun, but the outer portion of the disc it seems quite impossible to define, so that the picture of a spot shall be

* My latest experience has shown me that in using a highly bromized collodion for this instrument it is very necessary to be careful that the plates do not stay too long in the sensitizing solution. With this precaution, I believe, good pictures of the sun showing all the detail the instrument can give are easily obtained.

reasonably sharp. If this cannot be remedied with a lens of so short a focus, then possibly it might be desirable to increase the whole size of the instrument, and use a longer focal length in the enlarging lens, and failing this, I fear, the long focus lenses used by the American parties will have a great advantage.

If the Photoheliograph is to be employed at stations near the contacts in 1882, I would point out that this defect is of the very greatest importance. At these places I would use some form of Dr. JANSSEN's plateholder revolving in five minutes, and giving a picture every five seconds, and thus record cusp distances from which the actual contacts could be deduced, and also measures of the planet's distance from the limb. The distances to be measured being comparatively small, the scale might, I think, be inferred with sufficient accuracy from pictures of the whole solar disc when the planet is on it, but to make this arrangement hopeful, I conceive that the greatest sharpness of definition and freedom from distortion is essential.

In any case the moments of exposure should be automatically registered. This was done at Roorkee with the square plates in the following way: blades forming a pair of shears were attached to the two parts of a signal key of the pattern sent out for use with the chronograph, these being used to cut the cord, on severing which a slit flashed across the field and gave the exposure; the same pressure on the key thus cut the thread and made the contact which marked on the chronograph. The manner in which the exposures on the JANSSEN's plates were made to record themselves is shown in Mr. DE LA RUE's account of the dark slide in Royal Astronomical Society's Monthly Notices, Vol. XXXIV, p. 347. I do not think the above arrangement for recording exposures of the square plates perfect, for it was hurriedly got up and not very well executed; but well made it would be quite efficient in giving a better record than could be got from a chronometer

EQUATOREAL INSTRUMENT.

This was in my own charge. It is by Messrs. COOKE and SONS of York, of 6 inches aperture and about 82 inches focal length, and being, I believe, of their usual pattern requires no particular description. The main instrument with the usual fittings (except a parallel wire micrometer) and a double image micrometer by Mr. SIMMS reached me towards the end of October. Its shelter was ready, being a circular building 15 feet in internal diameter covered by a revolving roof: an opening 30 inches wide with shutters gives the means of examining any part of the sky from the zenith to the horizon.

No time was lost in putting this instrument in position, an operation in which I was greatly assisted by Captain CAMPBELL. Having adjusted it so that the polar axis pointed within a few seconds of the true pole, I at once set to work on the double image micrometer, an instrument of which I had no previous experience. I had asked for means of determining differences of right ascension and declination; these did not come with the instrument and it was doubtful if they would arrive in time (when they did arrive in the middle of December I found that it was impossible to use them) and I was thus obliged to take to an instrument which I had never used, and which I had never heard was even likely to come. Fortunately for the purposes of such work as the Transit of Venus it is singularly easy to manage, and the only trouble is the determination of scale, a process which I think it would not be difficult to facilitate very greatly.

The transit instrument, determination of micrometer scale, and measures of double stars for practice, kept me constantly employed in the evenings. The afternoons, after Captain STRAHAN's arrival on November 1, were devoted, whenever possible, to observations of the model of the Transit of Venus in order to determine our relative equations. It was impossible to use this model as at Greenwich, for the tube of the Cooke Equatoreal is only complete when it is mounted and there was no means of putting another telescope near it. I was obliged therefore to place

Captain STRAHAN'S (Simms') Telescope, whose tube admitted of it, on a temporary support outside the observatory and we had to adjust two heliostats at some distance behind the model, so that one threw the sun light into each telescope. The model and heliostats were placed on a building (the Magistrate's office) across the Meerut road, and the result was far from satisfactory. In any case a ray of light passing for some 140 yards not very far from the surface of the dry earth would be greatly disturbed in India, but to this was added the dust from traffic on the road, and the interruptions caused by clouds, and the losses of observations from one telescope having a good image, while the other was badly illuminated. Owing, too, to the distance of the telescopes it was absolutely necessary to interchange the observers, as there was a sensible equation due to the positions of the telescopes.

Captain CAMPBELL'S equation was determined at Greenwich by reference to Father SIDGREAVE and Captain BROWNE. It has been found here that I observe such a phenomenon as we saw in the model earlier than Captain CAMPBELL by 0·77*s*., and that Captain STRAHAN again observed it earlier than I by 0·34*s*., or 1·11*s*. earlier than Captain CAMPBELL. Also by direct comparison between Captains CAMPBELL and STRAHAN, it was found that the latter observed earlier by 1·08*s*.: showing, I think, that the comparisons fairly represented our estimates. But we saw no phenomenon of the same kind during the Transit itself, and I question if the comparisons are applicable: I have therefore not used them.

Of course, my first object was to observe the contacts as accurately as possible. The first external contact could not be observed, the Sun being too low, but soon afterwards I began measuring cusp distances.

Dial Time.			Micrometer Reading.	Assumed Zero.	Distance observed.		Refraction.	True Distance.
					Micrometer.	Space.		
h.	*m.*	*s.*	*rev.*	*rev.*	*rev.*	"	"	"
12	26	40	14·278	10·320	3·958	63·575	...	...
	27	40	6·404		3·916	62·901	...	...
	31	18	6·289		4·031	64·748	...	...
	32	58	13·987		3·667	58·901	...	...
	36	03	13·582		3·262	52·396	0·739	53·135
	37	40	7·396		2·924	46·967	0·623	47·590
	39	20	7·704		2·616	42·020	0·434	42·454
12	40	58	12·122		1·802	28·945	0·268	29·213

Only the last four observations have been used for deducing the contact internally, the others being taken at too great a distance from the contact. The micrometer measures have been converted into space by using the value of a revolution determined from a large number of transits of stars not far from the pole, which gave it $16{\cdot}0625'' \pm 0{\cdot}0132''$, and the refractions are computed by the formula of Professor CHAUVENET'S work.

The contact of limbs was observed at 12*h*. 42*m*. 30*s*. by dial. This dial was set by the Transit clock before observing, and was found to agree in the second after the Transit was over. At a convenient time during the Transit I marked seconds from the dial on the chronograph, which showed that the difference was only about 0·02*s*., the delay in marking and hearing compensating for the small amount by which the dial beat from its construction preceded that of the clock: I have considered the two practically identical. I had means for marking the time on the chronograph, but I could not use the chronograph key and the micrometer at the same time, and in this case I had not time to get the key, having continued observing cusp distances rather late. For this reason too I observed the contact with the double image micrometer which, with the first lens in use, gave a magnifying power of 128·4: the shade of dark glass (a wedge achromatized) was rather deep in order to reduce the haze round Venus, and the points of the cusps seemed very fine. The contact took place without any black drop or distortion: at the moment it occurred I was endeavouring to get hold of the tappet or key,* and in doing so, lost count, but I looked up at once at the dial (and allowed for the lost time), when I was sure that the critical phenomenon had really past without any of the peculiarities I had been led to expect.† This estimated interval was three seconds, and I think that all causes of error would be included in ascribing a probable error of two seconds to the observation of time.

* I may mention to save question, that my eye never left the telescope, and that this was the cause of my failing to get the key, as I was obliged to feel for it only.

† Mr. Stone has written a paper (Royal Astronomical Society's Monthly Notices, December 1876) which leads me to say that the time was not occupied from any uncertainty from penumbra, &c., but in scrutinizing to be sure that there was a complete break without any phenomenon such as I had expected to see.

The contact having passed, I proceeded to observe distances of the Planet's limb from that of the Sun with the micrometer, as in the following table:—

Time by Dial.			Micrometer Reading.	Assumed Zero.	Observed Distances.		Remarks.
					Micrometer.	Space.	
h.	m.	s.	rev.	rev.	rev.	"	
12	47	05	15·051	10·357	4·694	75·397	Distant Limb of ♀.
	49	22	5·446		4·911	78·883	
	50	30	9·289		1·068	17·155	After this are near limbs.
	51	26	11·546		1·189	19·098	
	·53	04	11·741		1·384	22·231	
	54	20	8·878		1·479	23·756	
	55	14	8·786		1·571	25·234	
	56	39	12·071		1·714	27·531	
	57	40	12·272		1·915	30·760	
	58	32	8·396		1·961	31·499	
	59	22	8·274		2·083	33·458	
12	60	51	12·537		2·180	35·016	
13	04	06	12·967	10·336	2·631	42·260	Focus slightly modified.
	5	20	7·551		2·785	44·734	
	6	08	7·446		2·890	46·421	
	7	45	13·384		3·048	48·959	
	8	40	13·493		3·157	50·709	
	9	49	7·161		3·175	50·998	
	10	54	7·060		3·276	52·621	
	11	58	13·868		3·532	56·733	
	13	10	13·924		3·588	57·632	
	14	24	6·616		3·720	59·753	
	15	07	6·447		3·889	62·467	
	16	22	14·258		3·922	62·997	
	17	23	14·352		4·016	64·507	
	19	54	6·181		4·155	66·740	

Some time was now spent in discussing the appearances at the contact and in sending a telegram to Captain STRAHAN at Lahore, warning him that probably he would not see the phenomena which had been seen in the model.

I then proceeded to take measures of the diameter of Venus. I had intended to measure only horizontal diameters, so as to have them free from refraction, and also to avoid, as far as might be, the tremor from the air which was rapidly warming; but after half a dozen measures, I gave this up, for the position of the micrometer required constant rectification and I found that this caused slight changes of *zero*. I took to measures of diameters in declination, as the direction of these was fairly near the horizontal: and 28 of these were measured half on each side of *zero*: I then measured 20 diameters in right ascension for a set, and afterwards measured another set of 20 measures. The results of these measures were communicated to the Royal Astronomical Society and published in their Monthly Notices, Vol. XXXV, p. 345: it is not necessary to give the individual observations here. When this work was completed, Venus was getting near enough to the Sun's limb to allow the distances of the limbs to be measured, and I proceeded to make the measures which are given in the following table:—

Time by Dial.			Micrometer Reading.	Assumed Zero.	Observed Distance.		Remarks.
					Micrometer.	Space.	
h.	*m.*	*s.*	*rev.*	*rev.*	*rev.*	"	
15	57	01	13·956		3·656	58·725	
	58	30	6·736		3·564	57·247	
	59	30	6·835		3·465	55·657	
16	00	32	13·664		3·364	54·034	
	2	15	13·466		3·166	50·854	
	3	40	7·308		2·992	48·059	
	5	32	7·493		2·807	45·087	
	7	07	12·973	10·300	2·673	42·935	
	8	04	12·842		2·542	40·831	
	9	02	7·916		2·384	38·293	
	9	46	7·907		2·393	38·438	
	10	33	12·535		2·235	35·900	
	11	20	12·520		2·220	35·659	
	12	22	8·276		2·024	32·511	
	13	01	8·372		1·928	30·969	
	14	11	12·176		1·876	30·133	

I was obliged to stop early, in order to see that the chronograph strip for my instrument was all right, to change the eye-piece, and prepare for warning the photographers of the approach of the internal contact.

For observing the internal contact, I used a Huyghenian eye-piece magnifying 204·5 times, with a smoke colored glass giving a yellow image of the Sun, which I had found to suit with the model, and to give a singularly clear contrast in the different parts of a sun spot. There was no black drop or distortion; the image was, indeed, dancing from the state of the air, but the cusps were quite fine, and I doubt if their thickness or that of the last relic of the solar limb exceeded the small amount due to the diffraction of the object glass. The contact was observed at 16*h.* 28*m.* 40*s.* by the dial, and I also marked on the chronograph for greater exactness, but unfortunately one of the strips of paper had, in Captain HEAVISIDE's short absence to observe with the Royal Society's Slater telescope, stopped the apparatus as I have before explained.

I then proceeded at once to change the eye-piece and observe measures of the chord joining the cusps. Unfortunately, changing the eye-piece and focussing takes time, and thus the most valuable cusp measures are lost. This circumstance, and also the few measures which are possible, while the distance is rapidly changing, are what have led me to believe that many good photographs would be very desirable—

Dial Time.			Micrometer Reading.	Assumed Zero.	Distance observed.		Refraction.	True Distance.
					Micrometer.	Space.		
h.	*m.*	*s.*	*rev.*	*rev.*	*rev.*	"	"	"
16	32	14	13·028	10·380	2·648	42·534	...	42·534
	33	13	13·274		2·894	46·485	...	46·485
	34	24	13·576		3·196	51·336	...	51·336
	36	14	13·900		...	...	...	...
	37	18	6·713		...	...	...	...
	38	14	6·603		...	...	...	...
	39	08	6·504		...	...	...	...
	40	00	6·415		...	...	...	...
	41	28	6·403		...	...	...	...
	42	17	14·393		...	...	...	...
	43	01	14·367		...	...	...	...
	44	04	6·343		...	...	...	...
	45	02	6·390		...	...	...	...
	45	54	14·276		...	...	...	...
	46	42	14·073		...	...	...	...
	47	30	6·560		...	...	...	...

The report of the chronograph stoppage caused a slight confusion, which made me measure all the early chords on the same side. Unfortunately too the later measures are too far from the contacts to be useful; but I have used them for getting an approximation to zero which is tested by the cusp measures. I have not thought it necessary to use refractions where the direction was so nearly horizontal and the space so small.

The last contact was noted at 16*h*. 55*m*. 44*s*. by the dial, and pricked at the same precise second on the chronograph. I have marked in my note book that "the error may be a second" for the impression of the Planet was very clear to the last, but on consideration I have preferred assigning a probable error of a second to the internal contact, and one of two seconds to the external. Notwithstanding atmospheric tremor, the vision was excellent, and the power and dark glass were as for internal contact.

As regards the measures: the best were certainly the diameters; next, the distances from limb, and lastly, the cusps: the great difficulty arose from the continual dancing of the images, parallel and perpendicularly to the line of separation, and, of course, this mainly affected the measures between points. I imagine that this difficulty would apply principally to a double image micrometer of this sort where each image is made by half the object glass. Where the two images are formed by a double refracting prism, they should be relatively steady, and if a satisfactory micrometer of this sort were to be had, it would be better for cusp measures, and probably for measures generally.

REDUCTION OF EQUATOREAL OBSERVATIONS.

The first step in this process was, of course, to get good interpolation formulæ. For this purpose the places and log distances of the Sun and Venus were taken from the Nautical Almanac for each Greenwich noon from December 5 to December 12, each co-ordinate being represented by a series $A + Bt + Ct^2 + Dt^3 + Et^4$, where the origin

of t was taken at 8.5d. and the unit was a mean solar day. The values of the co-efficients were then determined by the method of mean squares, which seemed as little laborious as any other procedure.* The formulæ obtained were then transformed for convenience into others where the origin of time was 12h. of my clock, and the unit one of its seconds.

On comparing the places from these formulæ with those given in the ASTRONOMER ROYAL's Tables, I found that the Sun's Right Ascensions were too small by 2·″03. I have repeated the deduction of the formulæ without finding any sensible difference from my first values, but the error must lie with me, for I have also computed the value from the Solar Tables directly, and find it close to the Greenwich value as does interpolation with second differences only. The other quantities I have found to agree with the ASTRONOMER ROYAL's values.

I have, therefore, corrected all my equations (as found in the computations) for this error, and they will be given here so as to be comparable with those deduced under SIR G. AIRY's supervision at Greenwich.

The general principles of the remaining calculations are those laid down by the ASTRONOMER ROYAL in his paper in Vol. XXXV, p. 277 of the Monthly Notices, of which he kindly furnished me with an early copy. In the formulæ I shall use the following symbols:—

Π is the mean Equatoreal Horizontal Parallax of the Sun.

p is the Horizontal Parallax of the body at the time and place.

Æ and δ are the Tabular Right Ascension and Declination.

τ the assumed sidereal time, and Θ the Tabular Hour Angle.

L is the Longitude East, ϕ the Astronomical, and ϕ' the Geocentric Latitude.

Accented letters denote apparent values.

The arbitrary corrections to the quantities are denoted by the sign Δ, while the changes by change of epoch are represented by differential co-efficients with respect to t.

Thus ΔÆ is the correction to the Right Ascension for errors of Tables, while $\frac{d\text{Æ}}{dt}$ is the motion in Right Ascension in one second of time.

The local sidereal time is thus $T + \Delta\tau$, and the complete value of Θ is—

$$\Theta = 15\tau + 15\,\Delta\tau - \text{Æ} - \Delta\text{Æ} - (\Delta\tau - \Delta L).\,\frac{d\text{Æ}}{dt}.$$

* This procedure is very laborious and appears needless. My reason for adopting it was as follows:—The Astronomer Royal placed great stress on places being computed "with the greatest precision" to 0″·01, and I saw no other way of readily getting general accordance to this extent. M. Leverrier's Tables give perturbations, &c., only to this amount, and the places of Venus would vary more than 0″·10 according to the mode of deduction.

The Parallax in Right Ascension has been calculated from the formulæ $P_{\text{Æ}} = p \cos. \phi' \sin. \Theta \sec. \delta$. Hence its complete value is $P_{\text{Æ}} + \cos. \phi' \sin. \Theta \sec. \delta \; \Delta p - P_{\text{Æ}} \cot. \Theta \; [15 \; \Delta\tau \; - \Delta\text{Æ} - (\Delta\tau \; - \Delta L) \; \frac{d\text{Æ}}{dt}]$. And

$$\begin{aligned} \text{apparent } \text{Æ} = \text{Æ} + P_{\text{Æ}} + \cos. \phi' \sec. \delta \sin. \Theta \; \Delta p + \Delta\text{Æ} \; (1 + P_{\text{Æ}} \cot. \Theta \sin. 1'') \\ + \Delta\tau \; [(1 + P_{\text{Æ}} \cot. \Theta \sin. 1'') \frac{d\text{Æ}}{dt} - 15 \; P_{\text{Æ}} \cot. \Theta \sin. 1''] \\ - \Delta L \; (1 + P_{\text{Æ}} \cot. \Theta \sin. 1'') \frac{d\,\text{Æ}}{dt}. \end{aligned}$$

The Parallax in declination is computed from the formulæ—

$$\tan. \gamma = \tan. \phi' \sec. \Theta. \qquad P_D = -p \sin. (\gamma - \delta) \operatorname{cosec.} \gamma.$$

To get the complete value we have—

$$\operatorname{Sec.}^2 \gamma. \; \Delta\gamma = \tan. \phi' \sec. \Theta \tan \Theta. \; \Delta \; \Theta = \tan. \gamma \tan. \Theta. \; \Delta \; \Theta.$$

$$\begin{aligned} \Delta\gamma &= \sin. \gamma \cos. \gamma \tan. \Theta \; [15 \; \Delta\tau \; - \Delta\text{Æ} - (\Delta\tau - \Delta L). \; \frac{d\text{Æ}}{dt}]. \\ &= \sin. \gamma \cos. \gamma \tan. \Theta \; [(15 - \frac{d\text{Æ}}{dt}) \; \Delta\tau \; - \Delta\text{Æ} + \Delta L \; \frac{d\text{Æ}}{dt}]. \end{aligned}$$

$$\begin{aligned} \text{Complete Par. in Dec.} &= -(p + \Delta p) \sin. \phi' \frac{\sin. \left(\gamma + \Delta\gamma - \delta - \Delta\delta - (\Delta\tau - \Delta L) \; \frac{d\delta}{dt}\right)}{\sin. (\gamma + \delta\gamma).} \\ &= -p \sin. \phi' \frac{\sin. (\gamma - \delta)}{\sin. \gamma} - \Delta p \sin. \phi' \frac{\sin. (\gamma - \delta)}{\sin. \gamma} \\ &\quad - p \sin. \phi' \frac{\sin. \delta}{\sin.^2 \gamma} \Delta\gamma. \sin. 1.'' + p \sin. \phi' \frac{\cos. (\gamma - \delta)}{\sin. \; \gamma} \\ &\quad [\Delta\delta + (\Delta\tau - \Delta L) \; \frac{d\delta}{dt}] \sin. 1.'' \end{aligned}$$

Substituting now the value of $\Delta\gamma$, omitting the term involving $\frac{d\text{Æ}}{dt}$ and also the last term of the complete value which is very small: we have—

$$\text{Complete Par. in Dec.} = P_D + \frac{\Delta p.}{p} P_D + p \sin. \phi' \sin. \delta \; \frac{\tan. \Theta}{\tan. \gamma.} (15. d\tau - d\text{Æ}) \sin. 1.''$$

in which $\Delta\text{Æ}$ may also be neglected.

$$\begin{aligned} \text{Thus appt. NPD} = \text{Tab. NPD} - P_D - \frac{\Delta p}{p} P_D \; - [\frac{d\delta}{dt} - 15 p \sin. \phi' \sin. \delta \; \frac{\tan. \Theta}{\tan. \; \gamma} \sin. 1''] \Delta\tau \\ \mid \Delta L \; \frac{d\delta}{\delta t}. \end{aligned}$$

$\Delta \, p$ has in every case been converted by the proper factor into $\Delta\Pi$.

The contacts and measures of distance of Limbs have been considered as variations of the same thing.

Taking Diff. Æ = appt. Æ ☉ − appt. Æ ♀

and Diff. NPD = appt. NPD ☉ − appt. NPD ♀ by the Tables we have—

$$\tan. \psi = \frac{\text{Diff. Æ}}{\text{Diff. NPD}} \sqrt{\sin.\ \text{appt. NPD} \odot \sin.\ \text{appt. NPD} ♀}$$

$$= A \frac{\text{Diff. Æ}}{\text{Diff. NPD}}$$

and $D' =$ appt. Distance of Centres = Diff. NPD sec. ψ.

If we call the sum of the terms which are added to the difference of Right Ascension as computed from the Tables ∇Æ; ∇NPD the corresponding quantity in NPD; $\nabla D'$ the consequent correction to D', and ψ and $\nabla\psi$ that to ψ.

Then we have $$\sec. \psi \nabla \psi = A. \frac{\text{Diff. NPD.} \nabla \text{Æ} - \text{Diff. Æ} \nabla \text{NPD.}}{(\text{Diff. NPD})^2}$$

$$\text{and } \nabla D' = -\frac{\sin. \psi}{\cos.^2 \psi} \text{Diff. NPD} \nabla \psi + \sec. \psi \nabla \text{NPD}$$

$$= - A \sin. \psi \nabla \text{Æ} + \cos. \psi \nabla \text{NPD}$$

and thus have a complete value of the distance of centres.

The true semi-diameter of the Sun S has been derived from the mean value used in the Nautical Almanac; or 961·82″ = [2·9830938].

The apparent semi-diameter—

$$S' = S. \frac{\sin. \Theta' \sin. \text{NPD}'}{\sin. \odot \sin. \text{NPD}} \text{ has been conveniently got}$$

by taking $$\log. S' = \log. S + \text{Par. in Æ} \frac{\text{change of log. sin. } \Theta \text{ for } 10''}{10}$$

$$+ \text{Par. in NPD} \frac{\text{change log. sin. NPD for } 10.''}{10}$$

For Venus I have used as a mean semi-diameter, the value determined by Mr. Stone, 8·472,″ which is near the value I have found by my own measures. It is unnecessary in this case to consider the difference of the true and apparent semi-diameters.

In the case of internal contacts, we have, of course—

$D' + \nabla D = S' + \Delta S - (\sigma + \Delta\sigma)$; σ being the semi-diameter of Venus.

For external contact $D' + \nabla D' = S' - \Delta S + \sigma + \Delta\sigma$

and if it be the measured distance of limbs—

$$D' + \nabla D' = S' + \Delta S - (\sigma + \Delta\sigma) - m.$$

The micrometer-measured distances of limbs have been taken in groups of four, the mean of the distances of the group being taken to correspond to the mean of the times of observation. By this means all uncertainty as to the slight changes of the micrometer zero have been avoided, and in the first group at Ingress I have eliminated the semi-diameter of Venus.

The following tables show the data used:—

NEAR INGRESS OF VENUS.

Mean Time by Dial.			Mean. Observed Distance.	Mean. Refraction.	Corrected Distance.	Remarks.
h.	m.	s.	"	"	"	
12	49	35·75	47·633	0·014	47·65	Centre of ♀ from Limb of ☉.
12	54	49·25	24·688	0·007	24·70	Distance near Limbs.
12	59	06·25	32·683	0·008	32·69	Ditto ditto.
13	05	49·75	45·594	0·011	45·61	Ditto ditto.
13	10	20·25	52·765	0·014	52·78	Ditto ditto.
13	14	45·75	60·712	0·015	60·73	Ditto ditto.
NEAR EGRESS.						
15	58	53·25	56·416	0·044	56·46	Distance near Limbs.
16	04	38·50	46·734	0·036	46·77	Ditto ditto.
16	09	21·25	38·366	0·029	38·40	Ditto ditto.
16	12	43·50	32·318	0·025	32·34	Ditto ditto.

But the measures of cusp distances have been differently treated. The rapidity and irregularity of the relative motion of the cusps render it impossible to group them, and I have availed myself of the facility with which each would give a value of the contact time, in order to eliminate sensible error in the zero, or rather to show that it does not exist.

Let $2c$ be the measured distance of the cusps,

$$\text{take} \frac{c}{\sigma} = \sin. Q \quad \frac{c}{S} = \sin. R.$$

and, internal contacts alone being necessary to be considered, we should have, if the data were all accurate—

$$\text{Dist. of Limbs} = X = (1 - \cos. Q)\,\sigma - (1 - \cos. R)\,S' = 2\,[\sigma \sin.^2 \tfrac{1}{2} Q - S \sin.^2 \tfrac{1}{2} R]$$

$$\text{also } \Delta X = (1 - \cos.Q)\,\Delta\sigma - (1 - \cos.R)\,\Delta S + \sigma \sin. Q\,\Delta Q - S \sin. R\,\Delta R$$

$$\text{and } \cos. Q\,\Delta Q = \frac{\Delta c}{\sigma} - \frac{c}{\sigma}\frac{\Delta\sigma}{\sigma} = \frac{\Delta c}{\sigma} - \sin. Q. \frac{\Delta\sigma}{\sigma}$$

$$\text{or } \Delta Q = \frac{\Delta c}{\sigma} \sec. Q - \frac{\Delta\sigma}{\sigma} \tan. Q$$

$$\text{similarly } \Delta R = \frac{\Delta c}{S} \sec. R - \frac{\Delta S}{S} \tan. R.$$

$$\text{Hence } \Delta X = (\tan. Q - \tan. R)\,\Delta c + (1 - \sec. Q)\,\Delta\sigma - (1 - \sec. R)\,\Delta S'$$

and the complete value of X is $X + \Delta X$.

In order to deduce the time of contact from the distance of limbs, we have first to compute the equations for the observed time of contact where the computed difference of centres contains a term in $\Delta\tau$., whose co-efficient is $\frac{d\,D'}{dt}$, whence we easily deduce $\frac{dt}{dD'}$, but we cannot consider that the motion is quite uniform. To get $\frac{d^2\,t}{dD'^2}$ we proceed as follows :—

$$D' = x^2 + y^2 = \sin. NPD' \odot \sin. NPD' ♀ (\text{Diff. } Æ)^2 + (\text{Diff. NPD})^2.$$

$$D' \frac{d\,D'}{dt} = x\,\frac{dx}{dt} + y\,\frac{dy}{dt}.$$

$$\left(\frac{d\,D'}{dt}\right)^2 + D' \frac{d^2\,D'}{dt^2} = \left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2 \quad \text{since the motions in Æ and Dec. may be considered constant.}$$

$$\text{Hence } \frac{d^2\,D'}{dt^2} = \frac{1}{D'}\left\{\left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2 - \left(\frac{d\,D'}{dt}\right)^2\right\}$$

$$\text{and } \frac{d^2\,t}{d\,D'^2} = - \frac{\frac{d^2\,D'}{dt^2}}{\left(\frac{d\,D}{dt}\right)^3}$$

$$\text{Then T (time of measure)} = T_0 \text{ (time of contact)} \pm (X + \Delta X)\,\frac{dt}{d\,D}$$

$$+ \tfrac{1}{2}\,(X + \Delta\,X)^2\,\frac{d^2\,T}{d\,D^2}$$

the upper sign being used at Ingress. And, if we neglect ΔX^2, we have

$$T_0 = T - X\frac{d\tau}{d\,D} - \tfrac{1}{2}\,(X)^2\,\frac{d^2\,t}{d\,D^2} - \Delta\,X \left\{\frac{dt}{d\,D} + X\,\frac{d^2\,t}{d\,D^2}\right\}$$

LAHORE STATION.

Lahore, the Capital of the Punjab, was chosen as a secondary station mainly because it was easily accessible by rail. To have gone further west would have entailed carriage by carts, which would have taken a considerable time, if the neighbourhood of Peshawar had been chosen; and, moreover, the further west we proceeded the greater seemed the chance of the whole observations being frustrated by clouds.

The station chosen by Captain STRAHAN was in the compound (or enclosure) of a house occupied by Dr. CALTHROP, commonly known as Mr. Elphinstone's house, and the property of His Highness the Maharajah of Kashmir: the house is about 500 yards, N. N. W. from Government House, and the data given by Captain STRAHAN will ensure the identification of the places of the pillars (even if they be destroyed) as long as the house stands, and with a little trouble at any time. The equatoreal pillar was connected by a traverse with a temporary station on a mound, called by Captain STRAHAN Donald Town Station, which, again, was connected with three stations (secondary) of the Great Trigonometrical Survey, and with the site of a fourth which had been destroyed. As the three known points are all spires of buildings, angles could not be taken at them, but Majáng Station (the destroyed one) is, Captain STRAHAN has informed me, on a mound so limited that it could not have been more than two or three feet away from the original position. I have verified the position of Donald Town Station as follows. Captain STRAHAN's observed angles give the means of deducing the positions both of his station of Donald Town and also the point he assumed to be Majáng from the three undoubted points of the Great Trigonometrical Survey, and consequently the distance and azimuth of Donald Town from Majáng Station admits of verification. I find—

1st.—Assuming that Captain STRAHAN'S is the true Majáng Station;

Donald Town from Majáng Station is 6517·1 feet at an azimuth at Majáng of 239° 49′ 05″.

2nd.—Interpolating Majáng Station, and thence deducing Donald Town;

Donald Town to Majáng Station is 6518·1 feet at the same azimuth.

3rd.—Interpolating Donald Town and thence deducing Majáng Station;

Donald Town to Majáng Station is 6520·1 feet, azimuth at Majáng 239° 48′ 57″.

There can thus be no sensible uncertainty as to the place of Donald Town.

From the mean of the last two values of the data and the Great Trigonometrical Survey data for Majáng Station, the position of Donald Town is deduced as—

N. Latitude	31°	33′	37·84″
E. Longitude...	74°	22′	39·45″

and since the traverse shows the equatoreal to have been distant from Donald Town Station 157 feet at an azimuth of 173° 17′, we have for the position of the equatoreal by the Great Trigonometrical Survey—

N. Latitude	31°	33′	38·51″
E. Longitude...	74°	22′	29·00″

The last quantity has to be decreased by 3′ 01·8″ to reduce the Great Trigonometrical Survey longitude to that which would be got by using, as a normal value, the present accepted longitude of Madras. Thus I get—

Geocentric Latitude ... 31° 23′ 23·5″ N.

Longitude ... 74° 19′ 27·2″ E. = 4*h.* 57*m.* 17·81*s.* E.

The height I have assumed from the best data I can get as 920 feet above the normal Ellipsoid of the earth, and hence ρ = 9·9996209.

Captain STRAHAN determined his time with a transit instrument of the Russian pattern belonging to the Great Trigonometrical Survey of

India. It is made by Messrs. COOKE and SONS, and the vision is through one end of the axis, the converging rays from the object glass being reflected down the axis. The aperture was about three inches. Captain STRAHAN reports the instrument very convenient to work with, but something led him to believe that the collimation correction was not constant, and he made a series of determinations throughout a day, of which the result seems to be that the position of the optical axis on the micrometer frame for the wires depends on the temperature. It may well be so, as it is hardly possible to conceive any mode of attaching a reflector in the middle of the axis by which the elasticity of metal shall not be brought into play, but while Captain STRAHAN'S observations fully establish the variability of the reading of the optical axis, they can, I think, hardly be considered as exhausting the subject and proving temperature to be the sole cause. The level correction is determined by the application of a level to the pivots; these have been assumed to be equal, as it was found that there was no considerable inequality; it would be an improvement to observe the wires by reflection. A 7-inch theodolite, by Messrs. COOKE and SONS, was enclosed in a blackened wooden box and used as a collimator. The Transit pier was 18 feet east of the equatoreal pillar.

Captain STRAHAN was furnished with two Chronometers, a Solar and a Sidereal. The Solar Chronometer was kept unmoved, and in as nearly a constant temperature as possible, in order to furnish a check on the Sidereal Instrument which was used in the Observatory where it was exposed to considerable changes of temperature, but the comparisons made three times a day and the rates from Transits unite to show that the Sidereal Chronometer was quite trustworthy, and I have consequently neglected the check one.

The following table shows the determinations of Chronometer correction to Sidereal time, and will be sufficient. I do not enter here into detail as to the determination of instrumental errors, because the only one as to which any doubt could exist is that of collimation, and in such an instrument it would be difficult to give the means of re-determining the error without drawings showing the parts of the instrument, or detail descriptions such as I have not now the means of making; but the deter-

minations have been carefully deduced and verified by Captain CAMPBELL and myself:—

Date.			Chronometer Time.	Correction.	Change between Observations.	Rate per day.
			h. m.	s.	s.	s.
December 1	...	...	1 21·2	+ 16·87	...	...
" 2	...	...	1 29·2	23·92	+ 7·05	+ 7·01
" 3	...	...	1 29·2	31·48	7·56	7·56
" 4	...	...	1 13·8	39·42	7·94	8·03
" 5	...	...	1 44·4	47·16	7·74	7·58
" 6	...	...	1 28·8	54·53	7·37	7·45
" 7	...	...	1 44·2	62·17	7·64	7·56
" 8	...	...	4 21·7	71·04	8·87	8·00
" 9	...	...	1 34·9	78·00	6·96	7·87
" 10	...	...	1 45·5	85·33	7·33	7·28

The individual observations are very good; and the effect of the chronometer rate is seen in looking over the individual determinations of the correction by the separate stars. On December 16, an attempt was made to determine the difference of Captain STRAHAN's habit of observing from mine, each of us taking a set of Transits of the same stars with the instruments and time-keepers, &c., as when working separately; but on examining the comparisons of the chronometers and clock, it was found impossible to clear up satisfactorily an error in recording the minutes of comparisons between the solar and sidereal time-keepers. I am sorry therefore that I cannot give the result; but it is evident that Captain STRAHAN's peculiarity of noting time must have affected his Transit Observations just as they did those of the contacts.

For observing the contacts of the Transit of Venus, Captain STRAHAN was supplied with a 6-inch telescope by Mr. SIMMS, which I had procured for the Total Eclipse at Dodabetta in 1871. For this I

had had an equatoreal mounting made at the Roorkee Workshops, which, though very rough and very heavy, was steady. A roughly-graduated circle (for cutting the divisions on which I am indebted to the Mathematical Instrument Department in Calcutta), read by two Verniers, gave the means of adjusting the polar axis to its proper position. A handle with a Hook's joint enabled the observer at the eye-piece to keep an object in the field, and another gave slow motion in declination: each handle actuating an endless screw working in a racked circle.

Some days before the Transit, experiments were made as to the best vision obtainable of spots on the Sun: it was found that an eye-piece magnifying about 125 times and applied with a plane solar reflector to the whole aperture gave the best results. Mr. SIMMS had sent an eye-piece supplied with means for correcting the dispersion at low altitudes and which was of higher power; this I had intended to be used, but the telescope stand was not ready till it had to be packed up, and we had no opportunity of trying the eye-pieces in position at Roorkee. On trying this, it was found that when the solar reflector was interposed, it would not come to focus; it was then too late to remedy what could easily have been corrected if known in time.

Captain STRAHAN after stating the conclusions arrived at as to suitable arrangements for vision, says of the observations:—

"Having assured myself on these points, and knowing that my "attention would not be distracted by having any measurements to make, "I awaited the phenomenon without much anxiety. The weather on "the morning of the 9th was most favorable. Ingress so far up-country "as Lahore was not visible: and for about an hour after sunrise, the "limbs of both the Sun and Venus were trembling considerably; but as "the Sun got higher, the definition became better, till, about 15 minutes "before contact, the edges of the two bodies were as hardly and sharply "defined as the most sanguine observer could have wished.

"Your telegram had prepared me for the absence of the black drop, "but not entirely for the appearance eventually seen. As the planet "moved towards the Sun's limb, she appeared to push away his edge "before her, the cause of which became evident in a few seconds; the "planet's edge was, in fact, encircled by a ring of light nearly as bright

" as the Sun, which prevented any contact, properly so called, from taking " place at all. The moment I have assumed for internal contact, and " recorded as such, is No. 2 in the Sketch (not reproduced), when the " Sun's light, unbroken by the ring of light, would just have grazed the " limb of Venus: This appears in my note book as 'slight darkness " between the limbs.' I have considerable confidence in the accuracy of " this observation, as the limbs were beautifully steady and sharp, and no " distortion apparent. There was no vestige of anything that could be " called a black drop or ligament. The following* is a *verbatim* copy of " the hurried notes made at the time" (the times given are chronometer times) :—

" Slight darkness between the limbs at 16*h*. 13*m*. 22*s*.; some uncer-" tainty about the minutes;† seconds correct, no black drop; the darkness " enveloped more of the limbs without increasing in depth (of shade); no " dark ligament occurred at all. After contact the limb of Venus outside " distinctly visible, owing to bright line of light round enveloping two-" thirds of it. The part absent (nearly) being on the east side. At 16*h*. " 19*m*. the light was round three-quarters of the limb of Venus, confirmed " by two spectators; cusps absolutely and perfectly sharp. At 16*h*. 23*m*. " the edge of light diminished to half the circumference of the western " limb. The part of the planet outside the Sun was palpably darker " than the sky; dense black background being purplish. Its shape in " no way distorted, magnified or diminished. At 16*h*. 26*m*. the edge of " light was fading; at 16*h*. 31*m*. 15*s*. gone, traces occasionally seen again; " at 16*h*. 34*m*. 055*s*. edge again distinctly visible; at 16*h*. 37*m*. it extended " round two-fifths of the circumference on the same part of the limb as " before; at 16*h*. 38*m*. 15*s*. visible as a glimpse only. External contact " and total disappearance at 16*h*. 41*m*. 09*s*."

" An elaborate discussion of these observations would, perhaps, be " out of place in this Report; but I am, nevertheless, tempted to offer a " few remarks on appearances so unexpected. There can be little doubt " that they point to the existence of an atmosphere round the planet, and " there is certainly no *a priori* improbability in such an explanation. It " is difficult to account for the position of the strongest part of the ring

* Captain Strahan here gives his correction of the chronometer, which is erron eous.

† Calculation shows the minutes correct.

"of light being unsymmetrically situated with regard to the line joining "the apparent centres of the Sun and Venus; but this is established "beyond all doubt—indeed, the most unpractised eye must have noted "the circumstance. It will be observed that the brightest part of it "is almost exactly on the preceding portion of the disc reckoning along "the line of the planet's motion; but whether this is a mere coincidence "or a significant fact, is not readily apparent. The ring was visible up "to the time of external contact, which enables one to make a rough "estimate of the refractive power of the planet's atmosphere; inasmuch "as the minimum (?) duration of a solar ray reaching the observer's eye "after refraction when Venus is at exterior contact must evidently be the "apparent diameter of Venus as seen from the Earth + the apparent "diameter as seen from the Sun. This deviation, in the present case, "amounts to about 1′ 27″."

As regards this last remark, the refraction deduced is but a small part of what has been got before by other means.

From the corrections to, and rates of, the chronometer given before, I deduce the following sidereal times of observation :—

		h.	m.	s.
Internal Contact—By Chronometer	... ...	16	13	22
Correction	... ...	+	1	14·93
Lahore Sidereal Time	... ...	16	14	36·93

		h.	m.	s.
External Contact—By Chronometer	... ...	16	41	09
Correction	... ...	+	1	15·08
Lahore Sidereal Time	... ...	16	42	24·08

The reductions have been made in precisely the same way as those at Roorkee; save that I have converted these sidereal times into Greenwich mean time and used that for calculating places in the series.

In revising these calculations I found that I had inadvertently used a longitude 4 seconds (or one minute of space) too small. The equations, as quoted in the following results, have been corrected by the use of the coefficient of ΔL and are now accurate.

RESULTS.

For the external contact at Ingress there are no observations. For the internal contact I have the following:—

1st.—The results of hour measures of the cusps near internal contact at Roorkee.

2nd.—The observed internal contact at Roorkee.

3rd.—The measures of distance of limbs near contact.

Of these, it will be convenient to leave the cusp measures to be considered last—

1st.—Observed Contact.

The observed time was *12h. 42m.* 30·0*s.* ± 2·0*s.*, whence I deduce the equation—

$$\begin{aligned} 7''.58 \pm 0''.07 = {} & -0''.0335\Delta\tau + 0''.0335\Delta L - \Delta S + \Delta\sigma + 0.6418\Delta \text{Æ}\,\text{♀} \\ & -0.7182\Delta NPD\,\text{♀} + 0.3338\Delta\Pi \end{aligned}$$

2nd.—Observed Distances of Limbs.

From these I deduce the following six equations of equal weight:—

$$\begin{aligned}
6''.23 &= -0''.0318\Delta\tau + 0''.0319\Delta L - \Delta S \phantom{{}+\Delta\sigma} + 0.6233\Delta \text{Æ}\,\text{♀} - 0.7371\Delta NPD\,\text{♀} \\ &\qquad + 0.2184\Delta\Pi \\
7''.18 &= -0''.0306\Delta\tau + 0''.0307\Delta L - \Delta S + \Delta\sigma + 0.6091\Delta \text{Æ}\,\text{♀} - 0.7509\Delta NPD\,\text{♀} \\ &\qquad + 0.1315\Delta\Pi \\
7''.13 &= -0''.0295\Delta\tau + 0''.0296\Delta L - \Delta S + \Delta\sigma + 0.5969\Delta \text{Æ}\,\text{♀} - 0.7623\Delta NPD\,\text{♀} \\ &\qquad + 0.0588\Delta\Pi \\
6''.11 &= -0''.0278\Delta\tau + 0''.0279\Delta L - \Delta S + \Delta\sigma + 0.5771\Delta \text{Æ}\,\text{♀} - 0.7801\Delta NPD\,\text{♀} \\ &\qquad - 0.0570\Delta\Pi \\
6''.51 &= -0''.0266\Delta\tau + 0''.0267\Delta L - \Delta S + \Delta\sigma + 0.5632\Delta \text{Æ}\,\text{♀} - 0.7919\Delta NPD\,\text{♀} \\ &\qquad - 0.1358\Delta\Pi \\
5''.69 &= -0''.0254\Delta\tau + 0''.0255\Delta L - \Delta S + \Delta\sigma + 0.5491\Delta \text{Æ}\,\text{♀} - 0.8034\Delta NPD\,\text{♀} \\ &\qquad - 0.2139\Delta\Pi
\end{aligned}$$

On looking over these results it is evident that the discordances are mainly due to errors of observation. I have therefore after some consideration preferred to deduce a probable error of result from these discordances. Taking the mean equation thus, I find—

$$6''.47 \pm 0''.23 = -0''.0286\Delta\tau + 0''.0287\Delta L + 0.833\Delta\sigma - \Delta S + 0.5865\Delta Æ\,♀ \\ - 0.7710\Delta NPD\,♀ + 0.0003\Delta\Pi.$$

3rd.—The cusp measures.

For the time of contact, calculated as I have explained from each of the cusp measures, we get the following values :—

h.	m.	s.				
12	42	44.6	$+41.480\Delta c$	$-22.461\Delta\sigma$	$+0.011\Delta S$	
	42	42.0	$+31.270\Delta c$	$-14.215\Delta\sigma$	$+0.009\Delta S$	
	43	10.0	$+25.084\Delta c$	$-9.738\Delta\sigma$	$+0.006\Delta S$	(rejected).
	42	39.3	$+14.671\Delta c$	$-3.643\Delta\sigma$	$+0.003\Delta S$.	

It seems probable that the time in the third measure has been noted wrongly half a minute: I prefer to reject this rather than apply any conjectural emendation. If there were any error of zero, then a sensible value of Δc would exist, and the sign would differ in the central observations from that in the first and last. It is manifest that there is no such correction needed. In order to weight these equations, I assume that one measure is subject to a probable error of half a second, which then is the probable value of $2\Delta c$. Hence the probable errors of the retained values are 10.8*s*., 7.8*s*., and 3.7*s*., and their weights may be taken as 1, 2 and 9 respectively. Hence the mean of the values will be—

$$12h.\ 42m.\ 40.2s. \pm 3.2s. - 6.973\Delta\sigma + 0.005\Delta S.$$

As the observed time of contact was 12*h*. 42*m*. 30.0*s*. we shall get an approximate equation for this time by substituting in the contact equation $+10.2s. \pm 3.2s. - 6.973\Delta\sigma + 0.005\Delta S$ for $\Delta\tau$ and restoring the term in $\Delta\tau$ which gives—

$$7''.92 \pm 0''.11 = -0''.0335\Delta\tau + 0''.0335\Delta L + 1.234\Delta\sigma - 1.000\Delta S + 0.6418\Delta Æ\,♀ \\ - 0.7182\Delta NPD\,♀ + 0.3338\Delta\Pi.$$

I am very doubtful if the equations from micrometer measures and those from actual observation of contact should fairly be combined into one equation, but if we do so combine them, we have for their weights—

Measures of limb distances	... ...	1
" cusp "	... ...	4
" observed contact	... ...	11

and the mean equation will be—

$$7''\cdot56 \pm 0''\cdot06 = -0''\cdot0332\Delta\tau + 0''\cdot0332\Delta L - \Delta S + 1\cdot068\Delta\sigma + 0\cdot6401\Delta Æ\,♀ - 0\cdot7197\Delta NPD\,♀ + 0\cdot3263\Delta\Pi.$$

This contact was also observed by Mr. Hennessey at Mussoorie, and the data are fully given in Proceedings of the Royal Society, Vol. XXIII, p. 379. I have computed a similar equation from these data; only assuming as the height of the station 6,850 feet, (for the same reason that I have increased the levelled height of Roorkee) to get the increase to the Radius Vector of the Normal Ellipsoid. I deduce the following equation :—

$$1''\cdot30 \pm 0''\cdot12 = -0''\cdot0342\Delta\tau + 0''\cdot0343\Delta L - \Delta S + \Delta\sigma + 0\cdot6496\Delta Æ\,♀ - 0\cdot7099\Delta NPD\,♀ + 0\cdot3564\Delta\Pi.$$

It will be seen that the observation by Mr. Hennessey differs a great deal from mine, and can only be nearly reconciled with it by supposing an error of *three minutes of time* in one of the Records. Now, I think, that the accordance of the residuals from the various classes of observations may be considered to be sufficient reason for trusting my own work; but I have conclusive evidence, I think, that the actual contact did take place at the time I have noted very nearly. From Captain CAMPBELL's Alt-Azimuth Observations I have deduced errors of the tables in Æ and NPD. These give the following corrections of the tables :—

$$\Delta Æ = 6''\cdot502.$$
$$\Delta NPD = -2''\cdot247.$$

Also Mr. Dunkin states that the value of ΔS when the mean semidiameter of the Sun is assumed $961''\cdot82$ is $-0''\cdot53$ from the Greenwich Observations (Royal Astronomical Society's Monthly Notices, Vol. XXXIII, p. 294), and I measured the mean diameter of Venus as $63''\cdot948$, whereas in computing I have deduced from Mr. STONE's value $64''\cdot10$; thus $\Delta\sigma$ would be $-0''\cdot076$.

L

Substituting these values in the above equations of condition for contact at Roorkee and Mussoorie, we get—

At Roorkee ... $1''.34 = -0''.0335\Delta\tau + 0''.0335\Delta L + 0.3338\Delta\Pi$.
At Mussoorie ... $-4''.97 = -0''.0342\Delta\tau + 0''.0343\Delta L + 0.3564\Delta\Pi$.

The discordance at Mussoorie seems unaccountable by any hypothesis but that I have suggested. I may add that the published time is only 23 seconds later than that which is deduced from the Nautical Almanac formulæ of prediction, in which agreement it is, I believe, singular. Mr. HENNESSEY assures me that there can be no error in the record of observation, and I am quite at a loss to explain the phenomenon.*

At the egress of Venus from the Sun's surface.—I have, as at the ingress, three classes of observation near the internal contact.

From the measures of limb distances I get the following equations of condition which as before have equal weight:—

$$3''.20 = 0''.0271\Delta T - 0''.0275\Delta L - \Delta S + \Delta\sigma - 0.1454\Delta Æ♀ - 0.9875\Delta NPD♀ - 2.2264\Delta\Pi.$$
$$3''.04 = 0''.0287\Delta T - 0''.0290\Delta L - \Delta S + \Delta\sigma - 0.1679\Delta Æ♀ - 0.9833\Delta NPD♀ - 2.2291\Delta\Pi.$$
$$2''.92 = 0''.0298\Delta T - 0''.0303\Delta L - \Delta S + \Delta\sigma - 0.1858\Delta Æ♀ - 0.9795\Delta NPD♀ - 2.2275\Delta\Pi.$$
$$2''.73 = 0''.0307\Delta T - 0''.0311\Delta L - \Delta S + \Delta\sigma - 0.1984\Delta Æ♀ - 0.9766\Delta NPD♀ - 2.2245\Delta\Pi.$$

The mean value is—

$$2''.97 \pm 0''.10 = 0''.0291\Delta T - 0''.0295\Delta L - \Delta S + \Delta\sigma - 0.1744\Delta Æ♀ - 0.9817\Delta NPD♀ - 2.2269\Delta\Pi.$$

of which I have derived the probable error from the probable error of one measure as deduced from my measures of the diameter of Venus.

The observed internal contact gives the equation—

$$3''.06 \pm 0''.03 = 0.0344\Delta T - 0''.0349\Delta L - \Delta S + \Delta\sigma - 0.2551\Delta Æ♀ - 0.9610\Delta NPD♀ - 2.1889\Delta\Pi.$$

The cusp measures give as the values of contact time—

h.	*m.*	*s.*					*s.*	
16	28	28.7	$-24.567\Delta c$	$+9.561\Delta\sigma$	$-0.006\Delta S$	$\pm$	3.6	*wt.* 2.6
		34.9	$-29.064\Delta c$	$+12.772\Delta\sigma$	$-0.008\Delta S$	$\pm$	4.4	*wt.* 1.7
		26.8	$-36.704\Delta c$	$+18.753\Delta\sigma$	$-0.008\Delta S$	$\pm$	5.5	*wt.* 1.1

* Captain Campbell observed the contact within two seconds of my observation, but the small aperture he used, and the high probable error I should have assigned, has made me leave this observation alone.

of which the probable errors are derived from the assumption that a single measure of distance has a probable error of $0''\cdot3$.

The mean of these values is—

$$\begin{array}{l} h.\ m.\quad s. \qquad s. \qquad\quad s. \qquad\quad s. \\ 16\ 28\ 30\cdot3 \pm 2\cdot5 + 12\cdot444\Delta\sigma - 0\cdot007\Delta S \end{array}$$

and I derive as before the equation of condition—

$$3''\cdot39 \pm 0''\cdot09 = 0''\cdot0344\Delta T - 0''\cdot0349\Delta L + 1\cdot428\Delta\sigma - \Delta S - 0\cdot2551\Delta Æ ♀ - 0\cdot9610\Delta NPD ♀ - 2\cdot1779\Delta\Pi.$$

The weights of these three equations of condition are, 1, 11 and 1·2; and the mean equation resulting is—

$$3''\cdot08 \pm 0''\cdot03 = 0''\cdot0340\Delta T - 0''\cdot0345\Delta L + 1\cdot039\Delta\sigma - \Delta S - 0\cdot2506\Delta Æ ♀ - 0\cdot9621\Delta NPD ♀ - 2\cdot1908\Delta\Pi.$$

Captain STRAHAN's observation at Lahore gives—

$$3''\cdot23 = 0''\cdot0344\Delta T - 0''\cdot0349\Delta L - \Delta S + \Delta\sigma - 0\cdot2551\Delta Æ ♀ - 0\cdot9610\Delta NPD ♀ - 2\cdot2641\Delta\Pi.$$

I should think that the probable error of this observation of time might well be several seconds, but the class of observation is so different from mine that I cannot assign a probable error to it with any confidence.

From Mr. HENNESSEY's observation of this phase at Mussoorie I deduce the equation—

$$3''\cdot77 \pm 0''\cdot07 = 0''\cdot0344\Delta T - 0''\cdot0348\Delta L - \Delta S + \Delta\sigma - 0\cdot2542\Delta Æ ♀ - 0\cdot9612\Delta NPD ♀ - 2\cdot2052\Delta\Pi.$$

EXTERNAL CONTACT AT EGRESS.

For this I have only the eye observations of each station. These give—

1st.—At Roorkee.

$$4''\cdot88 \pm 0''\cdot08 = 0\cdot0399\Delta T - 0''\cdot0405\Delta L - \Delta S - \Delta\sigma - 0\cdot3404\Delta Æ ♀ - 0\cdot9294\Delta NPD ♀ - 2\cdot0584\Delta\Pi.$$

2nd.—At Lahore.

$$3''\cdot38 = 0''\cdot0400\Delta T - 0''\cdot0406\Delta L - \Delta S - \Delta\sigma - 0\cdot3424\Delta Æ ♀ - 0\cdot9285\Delta NPD ♀ - 2\cdot1492\Delta\Pi.$$

3rd.—At Mussoorie.

$$4''\cdot96 \pm 0''\cdot04 = 0''\cdot0399\Delta T - 0''\cdot0405\Delta L - \Delta S - \Delta\sigma - 0\cdot3405\Delta Æ ♀ - 0\cdot9294\Delta NPD ♀ - 2\cdot0725\Delta\Pi.$$

Captain STRAHAN's observation is discordant: probably this is due to the same cause which showed the bright line round Venus to him and none to me.

It will be seen that the Egress observations point to the necessity for a sensible decrease* in the semi-diameter of Venus, though Captain STRAHAN's observations at Lahore would make the semi-diameter very near what I measured it. I believe the solution of this small value must be sought in the irradiation, which would especially affect the observation of the last contact and make it occur too soon, while Captain STRAHAN's eye might have been guided by the fact that the disc of Venus was visible outside the Sun.

It is more difficult to explain the fact, that while the cusp measures point to a time of contact when to the eye the planet was entirely within the Sun's disc, the measures of limb distances point to one when the internal contact was visibly incomplete. A negative value of $\Delta\sigma$ would increase the discordance and appears inadmissible in this case. If the value of the micrometric screw were sensibly affected by temperature, we should probably find the measured distances too small, though the accordance of results would be greater if the measurements were somewhat increased. I see, however, no way of determining the effect of the Sun's rays on the scale, unless the temperatures of the object-glass and micrometer can be separately estimated: it is clear that a temperature coefficient, derived from a general change in temperature of the whole instrument, could not be relied on.

I am disposed to think, that where the spectroscope is not used, external contacts should be observed with a pale dark glass, so as to facilitate the seeing of Venus outside the Sun's limb, but that a smoke-colored glass (brown yellow) as deep in tint as is convenient should be used for internal contact to diminish, as far as may be, the irradiation and the haze round the planet.

* Allowing for the errors of place before given, I find for the semi-diameter of Venus—

By my direct observations...	30″·87
„ Mr. Hennessey's	31″·18
„ Captain Strahan's	31″·70

Hence the mean semi-diameters would be—

8″·160, 8″·242, 8″·379,

and the mean of all, 8″·260,

not far from Eucke's determination from the Transit of 1761.

Micrometer observations are certainly valuable as checks on the direct observation; whether they are sufficiently accurate and otherwise suitable for combination must be determined from a larger experience than that of one station, but the following conditions seem essential to give them the greatest value in future Transits:—

1st.—The plane glass reflector should not be cemented into its place, but capable of expanding and contracting freely independently of the brass surroundings.

2nd.—Means must be supplied for determining the temperatures of the object-glass and micrometer separately as a check on the scale value.

3rd.—As it is possible that any reflection may alter the working focal length of the object-glass, it would be desirable that the scale should be determined with the reflector *in situ*. If the first of these conditions be fulfilled, the plane glass might be temporarily silvered by Liebig's process, when it would be possible to use stars for determining the scale, while the removal of the silvering would make the reflector available as usual for reducing the Sun's brilliancy and heat.

THE ALT-AZIMUTH OBSERVATIONS AT ROORKEE.

These observations were so designed that if the photographic operations failed, there might still be a series of observations of the relative position of the Sun and Venus throughout the Transit. I had many reasons for doubting the full success of the photographic class of observations: their principle is of course sound, but the application of it requires the solution of several problems which I think can hardly be considered as having their difficulties cleared up even at this date: I had no means of entering on them myself, and it was impossible to get full information of what was the progress made elsewhere. From my previous knowledge of the Alt-Azimuth I had very high ideas of its probable accuracy when used as I have attempted to use it: I think now these were too great, and my present experience has very much raised my estimate of the accuracy to be attained by observations of contacts and micrometer measures, so that I should now doubt whether any other class of observations can compete with such as I took with the equatoreal when the circumstances are favorable, but the various discussions as to past Transits of Venus, and the accounts of Transits of Mercury (which I never had any opportunity of observing with good optical appliances) formerly threw great doubt on the adequacy of such observations for Parallax determination.

As it was in any case necessary to employ the times of observations in the reductions, I had determined to use these alone independently of the divisions on the instrument's limb, making the observations strictly differential. The accuracy with which the adjustments of a good Alt-Azimuth can be made and their errors determined seemed to me to give this instrument great advantages for making the requisite observations; and the instrument itself which was available was especially steady in all its adjustments; it was the new Great Theodolite made by Messrs. Troughton and Simms for the Great Trigonometrical Survey of India

under Colonel STRANGE's supervision. I had no time, as it turned out, to study the peculiarities of the instrument; but I was fortunate in Captain CAMPBELL, who was quite competent to work out all the details of my general idea.

The instrument is described (so far as is necessary) in the Proceedings of the Royal Society, Vol. XX, p. 317. It was mounted under a revolving dome on a brick pillar capped with stone, and a platform was constructed round the pillar to enable the Observer to reach the eye-end of the Telescope with facility. The errors of the vertical axis were very small and very steady for long times, so that it was considered safer to trust to the instrument than to introduce the uncertainty of level readings. The supports of the transit axis admit of no adjustment at all.

The neglect of reading the level along with the observations enabled far more work to be done during a given time, and it had incidentally another advantage: there was no necessity for a rigorous investigation of the value of the level scales. If this had been necessary, it is doubtful if it could have been carried out, as the levels were all found to have their runs greatly varying with the temperature and unequal in different parts, so that the difficulty of getting a sufficient number of observations at a low temperature would have been very great indeed.

Of course it was of the first importance to protect the instrument, while in use, from the direct action of the Solar rays, and for this purpose the following arrangements were made by Captain CAMPBELL. A disc of millboard, 24 inches in diameter, was stiffened with brass, and so fixed on to the object end of the Telescope that it could without difficulty be removed and re-attached. The weight of this of course disturbed the equilibrium about the transit axis, which was carefully restored by the weights provided for balancing. A thick screen of cotton cloth was then placed over the opening of the dome, so as entirely to prevent any rays from the Sun passing it except through a hole 12 inches in diameter made in it and which could be adjusted, by moving the whole screen, so as to allow the rays to fall only on the object-glass and surrounding millboard. A flap closed this hole when it was not wanted and completely shut out the Sun: the result was quite satisfactory. The millboard of course prevented the reversal of the Telescope while it was attached, and it thus

became necessary only to reverse occasionally instead of after each complete observation as would perhaps have been desirable, but this is of small importance in such purely differential work.

Great care was taken to take the transits of the Limbs of the Sun and Planet over the same parts of the wires, so as to remove any errors from their position being slightly erroneous.

All the observations were registered on the chronograph, on which one strip of paper was devoted solely to the instrument.

There were five vertical and five horizontal wires in the telescope whose intervals were duly determined: those of the horizontal wires by transits of α Persei over them when near its greatest eastern elongation, and those of the vertical wires by transits of small stars as near the pole as the instrument could be conveniently set and near the meridian.

All the chronograph records were read by Captain CAMPBELL and verified by him afterwards, and before he left me he had aided me in the computations as far as the bringing up of the wire intervals.

REDUCTIONS.

The general principle of calculation has been as follows:—

It has been assumed that the instrumental corrections were so small that they would not affect the differences of the times of transit of the Sun and Venus over the wires; any small effect would be further reduced by the occasional changes of face.

1st.—A very approximate time of transit of the centre wire has been obtained for the centre of each body from the transits of both its limbs over that wire with an allowance (in the case of the Sun) for the change in the rate of motion which could be inferred from the last computed observation;

2nd.—For these approximate times of transit, the apparent places have been computed in exactly the same way as for the reductions of the observations already discussed;

3rd.—The next step has been to compute the apparent zenith distance or azimuth, as the case might be, with factors for the correction of the resulting value for corrections to the time, longitude, mean solar parallax and (in the case of Venus) to the right ascensions and declinations of the tables;

4th.—By suitable formulæ each Transit of a limb over a wire has had deduced from it the corresponding time of Transit of the centre over the central wire and a mean value of this time has then been deduced;

5th.—By using the coefficient for a correction to time, the zenith distance or azimuth at the assumed time has been reduced to what it would have been if computed for the mean time of Transit of the centre, and

6th.—As the zenith distances or azimuths were by observation the same, the difference of the two computed values has been made = o, giving an equation of condition.

If we call the Apparent Right Ascension $Æ + \nabla Æ'$.
NP. Distance $NPD' + \nabla NPD'$.
Sidereal time $\tau + \Delta\tau$ and $\Theta' = \tau - Æ'$.
Also ϕ the Latitude,
and tan. $M =$ cot. NPD′ sec. Θ'.

we shall have—

$$\text{Appt. ZD} = \cos.^{-1} \frac{\cos.(\phi - M)\cos.NPD'}{\sin.\ M} + (15\Delta\tau - \nabla Æ')\frac{\cos.\ \phi\ \sin.\ NPD'\ \sin.\Theta}{\sin.\ \text{appt. ZD.}}$$
$$- \nabla NPD'.\ \frac{\cos.\ \phi\ \sin.\ NPD'}{\sin.\ \text{appt. ZD}}.\ (\cot.\ NPD'\ \cos.\ \Theta\ - \tan.\ \phi)$$

of which the first term is the Tabular Zenith distance which is used in the denominators of the other terms.

So also—

$$\text{Appt. Azimuth} = \tan.^{-1} \frac{\cos.\ \phi\ \tan.\ \Theta'}{\sin.\ (\phi - M)} - \frac{\sin.^2\ Az.\ \cos.\ \phi}{\sin.^2\ NPD'\ \sin.\ \Theta'}.\ \nabla NPD$$
$$+ (15\Delta\tau - \nabla Æ)\ \sin.^2\ Az.\ \sin.\ \phi\ [1 + \cot.\ Az.\ \cot.\ \Theta\ \text{cosec.}\ \phi].$$

In computing both these I have used in getting the co-efficients, the logarithms taken out for the computation of parallax which correspond to the *true* values instead of those for the apparent values.

For reducing the transits of limbs over wires, and the transits of centre over middle wire, I have used the following formulæ :—

1st.—For transits over the horizontal wires—

Let ζ_0 be the apparent Zenith distance at transit of a centre wire.

δ_0 and Θ_0 the Declination and Hour Angle at the same instant.

C the distance of the wire from the centre wire.

S the semi-diameter of the body.

$$\left.\begin{aligned} A &= 1 + 2\,p\,\rho\,\cos.\,\zeta'\,\sin.\,1'' \\ B &= 1 + 2\,p\,\rho\,\sec.\,\zeta'\,\sin.\,1'' \end{aligned}\right\}$$ where p is the Equatoreal Horizontal Parallax of the body.

$a = \frac{1}{15}\cdot\frac{dR}{dt.}$; $b = 15\cdot\frac{d\delta}{dt.}$; and $I =$ the interval of time between the observed transit and that of the centre at the centre wire.

Then—

$$I = \frac{\sin.\,\zeta_0}{15\,A\,\cos.\,\delta_0\,\cos.\,\phi}\cdot\frac{C \pm S}{(1-a)\sin.\,\Theta_0 - b\,(1-\tan.\,\delta_0\cot.\,\phi\cos.\,\Theta_0)}$$
$$-\frac{15}{2}\cdot\frac{\cos.\,\Theta_0 - B\cot.\,\zeta_0\,\mathrm{cosec.}\,\zeta_0\cos.\,\delta_0\cos.\,\phi\sin.^2\Theta_0}{(1-a)\sin.\,\Theta_0 - b\,(1-\tan.\,\delta_0\cot.\,\phi\cos.\,\Theta_0)}\cdot I^2\cdot(1-a^2)\sin.\,1''.$$

where the I approximately computed from the first term is used in the second.

2nd.—And for reducing transits over a side wire to the centre vertical wire, I have used the following formula :—

$$I = \frac{1}{15}\cdot\frac{C \pm S}{n[\cos.\,p\cos.\,\delta_0 - \sin.\,p\sin.\,\delta_0\cos.\,(\Theta_0 - \vartheta)] - (1-a)\sin.\,p\cos.\,\delta_0\sin.\,(\Theta_0 - \vartheta)}$$
$$+\frac{15}{2}\cdot\frac{\cos.\,(\Theta_0 - \vartheta)\sin.\,p\sin.\,\delta_0}{n[\cos.\,p\cos.\,\delta_0 - \sin.\,p\sin.\,\delta_0\cos.\,(\Theta_0 - \vartheta)] - (1-a)\sin.\,p\cos.\,\delta_0\sin.\,(\Theta_0 - \vartheta)}\cdot(1-a)^2 I^2\sin.\,1''$$

where $n = \frac{1}{15}\cdot\frac{d\delta}{dt}$; $\cos.\,p = -\sin.\,\mathrm{Az.}\cos.\,\phi$; and $\cos.\,\vartheta = -\tan.\,\phi\cot.\,p$.

The following Abstract of one of these calculations will make the procedure more plain: I have given the figures as affected by the error

in the Solar Right Ascension which, as I have explained, have been allowed for in the results in all other places :—

No. of Set.	Clock time of Zero.	Intervals from Zero.					Objects.
		A	B	C	D	E	
		s.	s.	s.	s.	s.	
No. 5. Horizontal Wires.	h. m. s. 13 05 11·0	0·01	7·73	16·29	25·93	34·86	Sun's 1st Limb.
		185·85	193·97	202·40	212·32	221·23	" 2nd "
		87·23	95·09	103·73	113·38	122·40	Venus' 1st "
		93·02	100·97	109·74	119·20	127·92	" 2nd "

Approx. Trans. ☉ over C was... 13h 07m 00s·3 by clock, or 13h 06m 56s·42 S. T.

Tab. Rt. Ascens. ☉ ... 255° 46′ 40″·36.

" Declination ☉ ... –22° 48′ 48″·61.

Appt. Rt. Asc. ☉ $= 255° 46' 47''·68 + 0·0459\Delta\tau - 0·0456\Delta L + 0·8200\Delta\Pi.$

" NPDist. ☉ $= 112° 48' 54''·32 + 0·0043\Delta\tau - 0·0041\Delta L + 0·6392\Delta\Pi.$

" Zen. Dist. ☉ $= 77° 24' 02''·93 - 10·5015\Delta\tau - 0·0347\Delta L + 0·9899\Delta\Pi.$

	h.	m.	s.	
Mean of Transits from 1st Limb	13	07	00·18	clock time.
" " 2nd "	13	07	00·35	
General Mean ...	13	07	00·27	

At which moment—

Appt. Zen. Dist. ☉ $= 77° 24' 03''·25 - 0·0347\Delta L + 0·9899\Delta\Pi.$

Approx. Trans. ♀ over C was ... 13h 06m 57s·7 by clock, or 13h 06m 53s·82 S. T.

Tab. Rt. Ascens. ♀ ... 255° 56′ 21″·89.

" Declination ♀ ... –22° 36′ 54″·40.

Appt. Rt. Asc. was ... $255° 56' 49''·18 - 0·0249\Delta\tau + 0·0261\Delta L - 3·0556\Delta\Pi + \Delta Æ.$*

* In strictness both ΔÆ and ΔNPD have a factor differing slightly from 1, but it has been thought unnecessary to consider them as different, since the errors of the places of the Sun and Venus cannot be separated.

Appt. NPDist. was ... $112^\circ\ 37'\ 15''\!\cdot\!61 - 0\cdot0125\Delta\tau + 0\cdot0132\Delta L + 2\cdot3749\Delta\Pi + \Delta NPD.$

„ Zen. Dist. „ ... $77^\circ\ 24'\ 01''\!\cdot\!80 - 10\cdot5965\Delta\tau + 0\cdot0269\Delta L + 3\cdot6871\Delta\Pi + 0\cdot7047\Delta Æ + 0\cdot6458\Delta NPD.$

	h.	m.	s.	
Mean of Transits from 1st Limb	13	06	57·74	clock time.
„ „ 2nd „	13	06	57·50	
General Mean ...	13	06	57·62	

At which moment—

Appt. Zen. Dist. ♀ $= 77^\circ\ 24'\ 02''\!\cdot\!65 + 0\cdot0269\Delta L + 3\cdot6871\Delta\Pi + 0\cdot7047\Delta Æ + 0\cdot6458\Delta NPD.$

Hence*—

$$0''\!\cdot\!60 = 0\cdot0616\Delta L + 2\cdot6972\Delta\Pi + 0\cdot7047\Delta Æ + 0\cdot6458\Delta NPD.$$

The observations were of three classes—

Complete Observations.—Where both limbs of both bodies were observed over all the five wires (whether Vertical or Horizontal) of the instrument.

Imperfect Observations.—Where both limbs of the Sun were observed over all the wires, while Venus was only observed in part, observations of one or both limbs over some wires being missing.

And a few observations which were rejected as too imperfect to be worth reducing.

It is manifest that the error of tabular semi-diameter could produce no effect on the complete observations, and that these would, except a few when the Sun was very low, all be of the same weight. But in the case of the imperfect observations it was necessary to consider that the diameter of Venus seen with the small aperture of the telescope might be very sensibly different from the assumed value, and that such a difference would affect the deduced time of Transit of the Planet's centre. Moreover, the observations would all be of less weight than the complete observations, and vary somewhat in weight.

* The corrections to time have been omitted.

The 30 complete observations were therefore reduced first, and the result showed that the apparent semi-diameter of Venus was in fact $1''\cdot50$ smaller than it had been assumed, the observations being very accordant, considering their class. This reduction was then applied to the Tabular value in computing the 8 imperfect observations, which may, I think, now be considered as free from any error from this cause.

I thus obtained 38 equations of condition involving the same correcions to the elements and place of observation as before, and also corrections to the mean times of transit of the bodies observed.

Of these there were 17 from complete, and 5 from imperfect, observations over the horizontal wires, and 13 from complete, and 3 from imperfect, observations on the vertical wires, the incomplete observations being about the middle of the Transit.

To form normal equations I have divided the complete observations, so far as they go, into groups of 5. Two complete and five imperfect observations on the horizontal wires have been combined to form one normal equation, and three imperfect observations in the middle of the Transit over the vertical wires have been combined with three complete ones near the end for another group.

I have thus obtained the following seven normal equations, in writing which I have here omitted the terms depending on correction, to the time :—

$$
\begin{aligned}
0\cdot7160\Delta Æ + 0\cdot6309\Delta NPD + 2\cdot7124\Delta\Pi + 0\cdot0621\Delta L - 1''\cdot79 &= 0 \quad (+1\cdot45)\\
0\cdot6770\Delta Æ + 0\cdot6796\Delta NPD + 2\cdot6573\Delta\Pi + 0\cdot0602\Delta L - 3''\cdot92 &= 0 \quad (-1\cdot05)\\
0\cdot6239\Delta Æ + 0\cdot7367\Delta NPD + 2\cdot5801\Delta\Pi + 0\cdot0573\Delta L - 2''\cdot20 &= 0 \quad (+0\cdot20)\\
0\cdot5050\Delta Æ + 0\cdot8335\Delta NPD + 2\cdot4336\Delta\Pi + 0\cdot0504\Delta L - 0''\cdot92 &= 0 \quad (+0\cdot49)\\
1\cdot0286\Delta Æ - 0\cdot3915\Delta NPD + 0\cdot0033\Delta\Pi + 0\cdot0667\Delta L - 7''\cdot33 &= 0 \quad (+0\cdot24)\\
1\cdot0349\Delta Æ - 0\cdot4424\Delta NPD + 0\cdot0037\Delta\Pi + 0\cdot0663\Delta L - 7''\cdot48 &= 0 \quad (+0\cdot24)\\
1\cdot0976\Delta Æ - 0\cdot3252\Delta NPD + 0\cdot0027\Delta\Pi + 0\cdot0728\Delta L - 8''\cdot25 &= 0 \quad (-0\cdot38)
\end{aligned}
$$

of which I consider the first equation to have only half the weight of the rest, owing to the Sun's low altitude.

From these in the usual way I get the following two equations for determining $\Delta Æ ♀$ and $\Delta NPD ♀$, which alone can be got without reference to other stations :—

$$
\begin{aligned}
4\cdot5645\Delta Æ ♀ + 0\cdot2361\Delta NPD ♀ + 5\cdot1334\Delta\Pi + 0\cdot3302\Delta L - 29''\cdot148 &= 0.\\
0\cdot2361\Delta Æ ♀ + 2\cdot2536\Delta NPD ♀ + 6\cdot1591\Delta\Pi + 0\cdot0558\Delta L + 3''\cdot528 &= 0.
\end{aligned}
$$

o

whence—

$$\Delta Æ ♀ = 6''.502 - 0.071 \Delta L - 0.989 \Delta \Pi. \quad p.e. = \pm 0''.312.$$
$$\Delta NPD ♀ = -2''.247 - 0.017 \Delta L - 2.629 \Delta \Pi. \quad p.e. = \pm 0''.439.$$

These probable errors are deduced by neglecting the terms in ΔL and $\Delta \Pi$, and the residuals on the same condition are given at the end of each equation in brackets.

From these I deduce the following corrections to the place of Venus in geocentric longitude and latitude—

$$\text{Corrn. to Longitude} = 5''.757 \pm 0''.290 - 0.067 \Delta L - 1.165 \Delta \Pi$$
$$\text{,, Latitude} = 2''.820 \pm 0''.438 - 0.011 \Delta L + 2.528 \Delta \Pi.$$

In which the Sun's place has been assumed to be accurate.

These values differ a little from those I have given elsewhere from my having gone over the whole solution of the equations again, finding a small error in work which I had been unable to get checked by an independent computer.

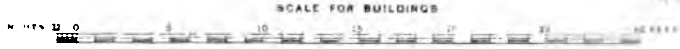
PLAN AND SECTIONS

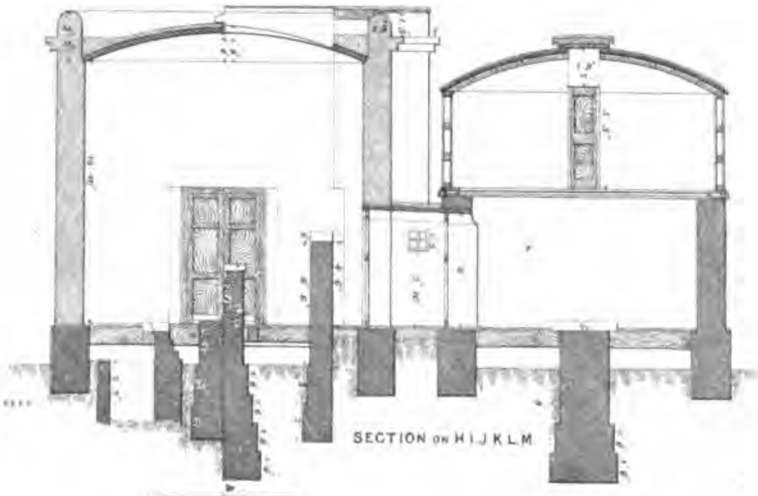
OF THE

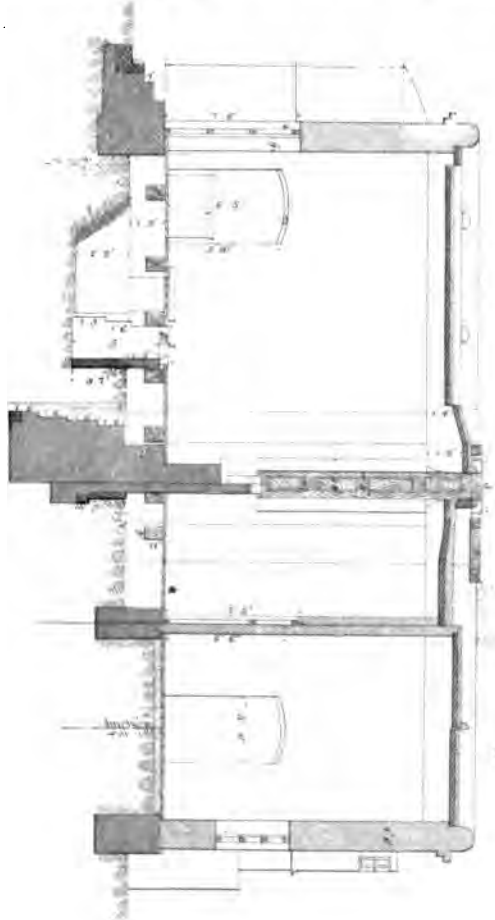
OBSERVATORY,

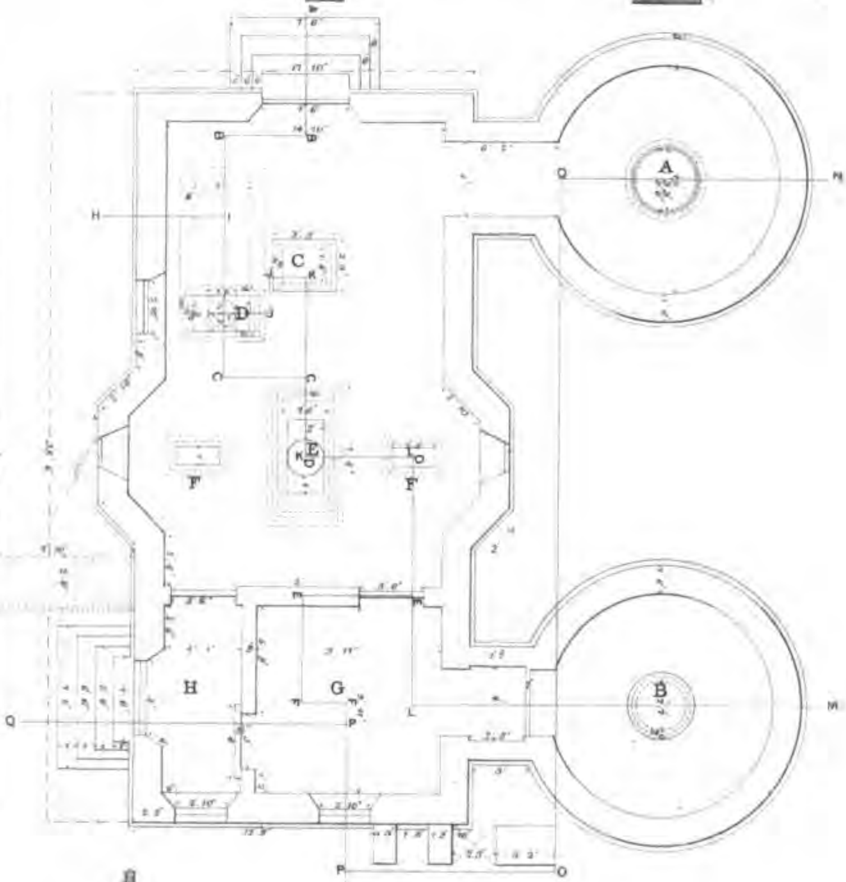
ROORKEE.

SCALE FOR BUILDINGS









REFERENCES.

A. .... Altazimuth

B. .... Photoheliograph

C. .... Clock

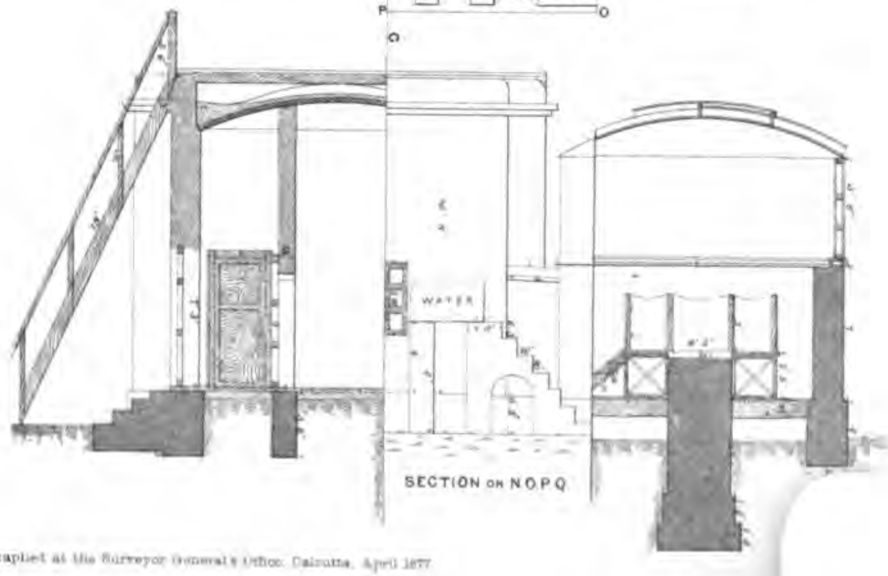
D. .... Chronograph

E. .... Transit Instrument

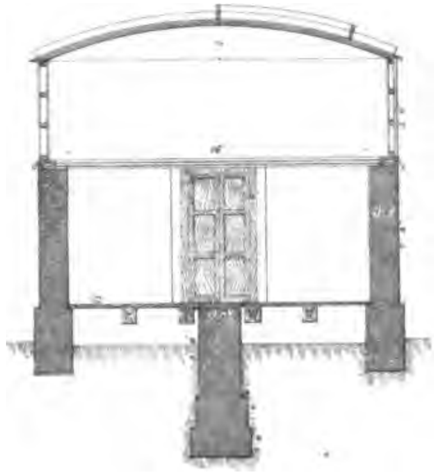
F. F. .... Collimators

G. .... Dark Room

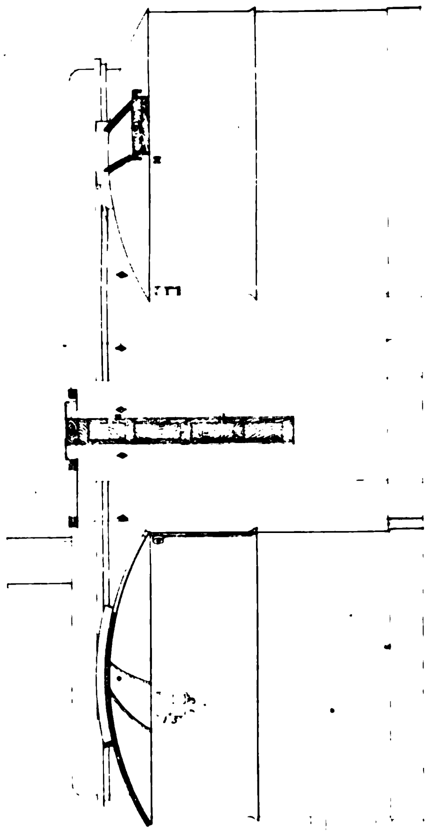
H. .... Entrance Passage



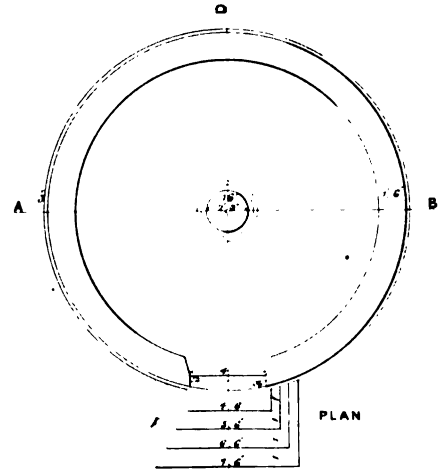
Lithographed at the Surveyor General's Office, Calcutta, April 1877



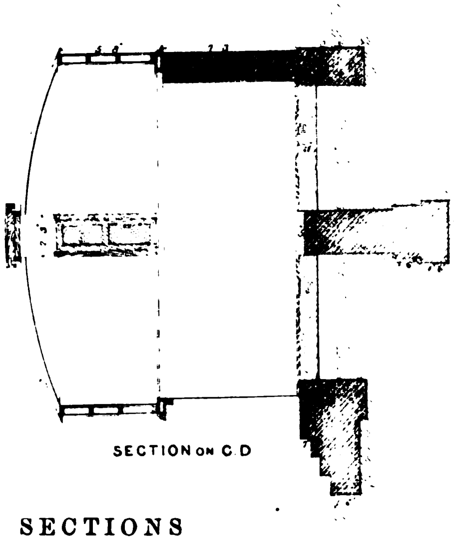
SECTION ON A B



ELEVATION OF OBSERVATORY LOOKING N.

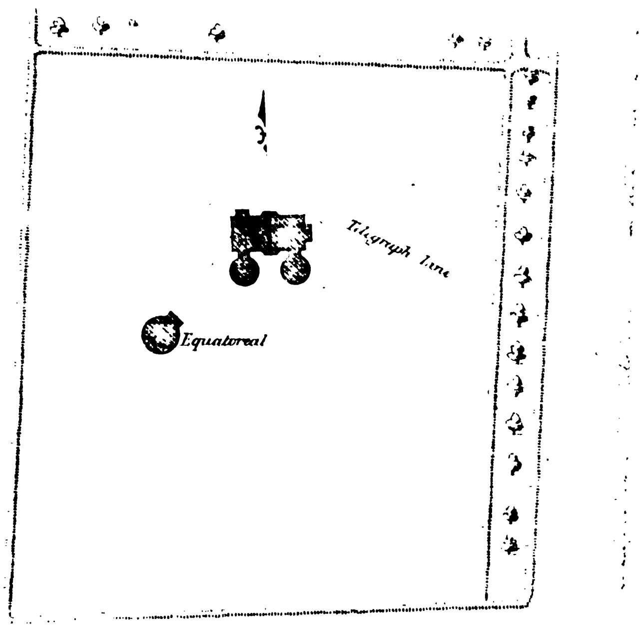


PLAN



SECTION ON C.D

PLANS AND SECTIONS OF LARGE TOWER FOR EQUATOREAL.



PLAN OF OBSERVATORY COMPOUND

SCALE

Lithographed at the Surveyor General's Office Calcutta April 1877

I. 6.73

SELECTIONS

FROM

THE PORTFOLIOS OF THE EDITOR

OF THE

LUNAR MAP AND CATALOGUE.

FIRST ISSUE.

LONDON:
PRINTED BY TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET.
1873.

SELECTIONS, ETC.

ADDRESS.

The increasing interest which is manifested in Selenography renders it important that Students, as well as the astronomical public generally, should be in possession of the results of each other's labours. With this view the illustrations in the following pages have been selected. The opening and succeeding articles have been written for the purpose of indicating special lines of research which it is believed will, if pursued systematically, lead to valuable results. There can be no doubt that the great question of PHYSICAL CHANGE, as regards the moon, is of absorbing interest; and it will greatly assist the earnest inquirer if he can bring to bear on this subject a competent knowledge of terrestrial chemistry and geology, combined with a determination of the reflective powers of the various rocks which characterize the earth's surface. The road most likely to end in the soundest conclusions is that which affords the most extensive views of terrestrial analogies.

[I.] ON THE STUDY OF CHANGE IN THE LUNAR SURFACE.

By the Rev. T. W. WEBB, M.A., F.R.A.S.

To those whose acquaintance with selenography comprises rather its general features than its minuter or more practical details, it may appear singular that the question of change on the surface of our satellite should remain as yet in so undecided and unsatisfactory a condition. It is not from any thing unattractive in the subject itself. There can be no hesitation as to the interest of the inquiry whether all in our neighbour-world is now dead and cold and still, or whether the mighty energies which once tore up and devastated its exterior may not yet be working, though with diminished activity—or of the kindred doubt whether the solid materials of that globe are in direct presentation to what is usually considered empty space, or are shielded by an interposed gaseous stratum, the probable aliment of some form of vegetable or animal life. It is not, again, from scarcity of observers or means of observation. These have of late been multiplied in a degree unprecedented in the history of astronomy. Nor has it been from restriction as to time. Nearly a century has elapsed since the inquiry was first seriously mooted by Schröter: during that interval how great has been the advance of other departments of astronomy; how many inquiries of apparently a less promising nature have arisen and been prosecuted with success; and yet this question stands much as it was left by the diligent old Hanoverian astronomer. Schmidt has indeed effectually reopened it of late by his very interesting discovery of the altered aspect of the spot *Linné*; and Birt and others, including the present writer, have pointed out localities where more perhaps than suspicion may be entertained; but, though many observers are sufficiently persuaded of the existence of variations of some kind independent of mere optical effect, others remain unsatisfied, or withhold their opinions. The inquiry, at any rate, is still very incomplete; and it may not be unsuitable to point out some of the causes which may have operated in checking for such a length of time the growth of this interesting investigation.

In the first place, the subject has been approached with an amount of prepossession which would inevitably retard the attainment of truth. That Schröter was one of the most diligent and careful of observers admits of no doubt, or that as far as singleness of purpose went, he could, as he himself asserts, have made affidavit of what he has recorded. But we are all liable to an unconscious bias; and in his case it may have been that his extreme interest in the almost untrodden path of discovery which he had chosen led him to overlook some necessary precautions, and to form some premature conclusions. His great successor, Mädler (for Beer is believed to have had little to do with actual observation), seems to have been at least equally biased in an opposite direction, and to have undertaken his task with a determination to ignore rather than to examine the previous evidence or supposed evidence of change. There is little risk in the assertion that the real progress of knowledge has been retarded in this, as it must be in every case, by such opposing prepossessions.

In the next place, we must bear in mind that the objects among which our search must lie are for the most part of very inconsiderable dimensions, and subject to corresponding difficulty of observation. This cannot, indeed, be affirmed without exception, as some of the supposed variations in reflective power are on a considerable scale; but these have been comparatively less regarded; while as to the traces of eruptive action, even the most zealous maintainers of its present continuance would hardly look for any extensive result. As far as existing delineations can guide us, it is only among the smallest class of craters or fissures or similar objects that we may hope to find evidence of change. Much of the past history of the moon is so legibly written in the obvious chronological succession, and correspondingly diminished magnitude, of the results of volcanic force, that there is little ground to expect any renewal of similar magnificently developed eruptions; and though it by no means follows that we have reached a period of absolute extinction, we have great reason to believe that we look upon a comparatively quiescent surface, and must be satisfied with tracing the feeble struggles of expiring power. And so with the atmospheric question: while it is deducible alike from theory and observation that our satellite is destitute of an aërial envelope bearing any comparison with our own, we are not justified in denying the possible, or rather probable, existence of something of an analogous nature; but from its extreme tenuity we must infer corresponding uncertainty of detection. In either case it obviously follows that our inquiry is thus limited in a way that greatly enhances its difficulty. Coarser drawings and inferior telescopes will do little for us here.

Again, we have to remember that the minute details of the lunar surface are subject to various causes of illusion in regard to their visibility. It would readily occur that in proportion to their deli-

reflect that light back to us at continually varying angles; the shadows through which their true relief is made known fall on a very irregular surface, and are thus subject to frequent accidental distortions; and the change of lunar seasons, though small in amount, is capable of altering the direction of the incident light and projected shadow quite sufficiently to give a fresh aspect to objects in suitable positions. From such causes alone many optical variations might be expected, and will be found to occur. But selenography would be an easier task if these were all. We meet with another very influential source of apparent change in libration. The additional unsteadiness, so to speak, of the landscape, the perpetual shifting of its inclination both to incident and reflected light, the constantly varying amount of perspective, and the slow recurrence of precisely identical conditions must necessarily add to the difficulty of accurate discrimination of details.

Such are some of the inevitable hindrances which have always opposed the rapid prosecution of our inquiry, even in the ablest and most careful hands; and these may suffice to account for the incomplete solution of the problem up to the present time.

We are not, however, entitled to infer from all this any thing unfavourable to future success. The causes of illusion being invariable and recurrent in their nature, their effects will disappear in a broad and continuous average: nor is too much under ordinary circumstances to be ascribed to them; for otherwise, as Schröter has justly remarked, we should be constantly witnessing variations in a single prolonged observation which are contrary to all experience. Careful and enduring attention will enable the patient selenographer to extricate his work from this apparent entanglement, to eliminate ... should be noted at the time; and we must be tolerant of uncertainty, and resolute against hasty conclusions. It must, after all, be strictly matter of evidence, and of such evidence as has borne rigid comparison and cross-examination. Photography has lately rendered most important assistance, but should not supersede actual observation: the artificial eye, though more comprehensive and unerring, is less keen and clear, as well as more limited in its opportunities, than the natural organ. Ultimately we may certainly venture to hope that, by the careful collection of many independent results, the effects of personal and instrumental equation, atmospheric indistinctness and optical variation will be so clearly distinguishable and separable that we shall be able to decide with certainty what the earlier observers could only infer with much risk of error, whether there is or is not an amount of apparent change not to be explained on any of these grounds. Should we be thus led to an affirmative conclusion, it may be expected to be of more than one kind; and this, while adding to the complexity, will certainly not diminish the interest of the inquiry. In some cases, as probably in *Linné*, there may be indication of alteration of form; in others, as in *Werner* and near *Picard*, we may trace change of reflective power; in others, as probably in some of the minute variations on the floor of *Plato*, we may suspect variable density in a superjacent gaseous stratum. These localities are alluded to as mere instances of an enumeration which might be largely extended on another occasion; at present there is neither opportunity nor space for such a full discussion of the observations of Schröter and others bearing on these several points as might at some future time do good service to the student in this interesting investigation.

[II.] REMARKS ON THE NATURE OF THE LUNAR SURFACE AND ENVELOPE.

By EDMUND NEISON, F.R.A.S., F.C.S.

Mr. Proctor, in his paper in the 'Quarterly Journal of Science,' January 1873, pp. 29–55, on the "Condition of the Moon's Surface," appears to touch too lightly on the primary question which presents itself when an inquiry into the history of the moon is commenced—namely, the nature of the surface, from the appearance of which we are mainly to draw our conclusions. It would appear more satisfactory, before endeavouring, as Mr. Proctor does, to ascertain what brought the moon's surface to its present condition, to determine what that surface really is.

When it is remembered that we are justly limited to conceiving only of the same elements in connexion with the moon as we know to exist on the earth, it will, we apprehend, be found possible to draw sound conclusions as to what this surface must be; otherwise we open a boundless field for the imagination, and are in danger of placing the whole subject beyond the pale of practical discussion. Arguing, then, from what chemistry demands, there can perhaps be no doubt that oxygen must enter as *extensively* into the constitution of the moon's surface as we find it does on the earth's; or the general appearance of the disk would, we might suppose, be of an entirely different character from what it is[a]. It is of course quite impossible to enter into any details on such subjects within the limits of this paper, even had it not required an acquaintance with the special branch of science concerned to understand them properly. Had not physical science most unmistakably asserted its general truth, probability would naturally lead to the conclusion that the primitive formations of both earth and moon were of the same character. And a careful consideration of the whole subject will necessitate the further conclusion, that the basis of the lunar surface consists of the mixed silicates of the same nature as terrestrial gneiss, granite, basalt, lava, &c.; and it is from this basis that we must build up our lunar formations, with due regard to its capabilities. Whatever, then, may have been the original condition of the matter now constituting the lunar surface, it must at some period have been intensely heated to have formed these silicates, to such an extent, in fact, as to have fused every thing and to have volatilized most.

From the nature of the surface thus determined from chemical considerations, it is possible to deduce with some degree of accuracy some important points with regard to the progress of the lunar surface to its present condition; and this will enable us to apply a test to several hypotheses that have been advanced. And here we find a little difficulty in the way of the hypothesis discussed by Mr. Proctor in his paper as to the immense influence brought to bear by

[a] ["Supposing the moon to be constituted of similar materials to the earth, it must be, to say the least, doubtful whether there is oxygen enough to oxidate the metals of which she is composed; and if not, the surface which we see must be metallic, or nearly so."—Sir William Grove's Address to the British Association, Nottingham, 1866, p. lx.

Sir William further suggests that it is a fair subject of inquiry to investigate lunar formations with the especial view of ascertaining if they present the appearance of "congealed metallic masses, as they may have set in cooling from igneous fusion." In connexion with this question we would solicit the reader's attention to Mr. Neison's view of the surface consisting of mixed silicates, and also to the cognate view of Mattieu Williams, founded on the fact that silica enters largely into the composition of the earth's crust. (Monthly Notices R. A. S. vol. xxxiii. p. 360.)

Professor Phillips remarks (Phil. Trans. 1868, p. 339), in relation to the difference of aspect of *Aristarchus* and *Herodotus*, on the dazzling brightness of *Aristarchus* as arising from its consisting of material similar to white trachyte, while the dark colour of *Herodotus* may be due to something analogous to basalt or to augitic compounds.—Ed.]

an hypothesis requiring such assistance.

Lunar Atmosphere.

It is to be regretted that Mr. Proctor practically waives the question as to a lunar atmosphere, and dismisses the subject as of little importance, being apparently of the general opinion that whether no lunar atmosphere, or one of a very limited extent exists, is a matter of indifference; but upon careful consideration it will appear that this opinion is entirely inadmissible. We have here matter for the gravest attention, and on the decision of this point conclusions of the most serious importance depend. If, for example, we decide against the absolute existence of any atmosphere, such as a thin gaseous envelope, we demand very improbable circumstances with regard to the lunar constitution; but if, on the contrary, we admit the existence of an atmosphere, however limited, it imperatively requires consideration as to how far this might effectually conduce to the same purposes as our own. It is therefore to be regretted that Mr. Proctor does not enter upon this consideration, but waives the entire matter, with the exception of assuming in certain points the inability of any lunar atmosphere to do this. As, however, we examine into our decision as to what consequences the existence of a lunar atmosphere carries with it, the subject gradually assumes a very different aspect; and while we see that non-existence, or a very limited existence, are not synonymous, it gradually becomes clearer that for an immense period a lunar atmosphere must have existed, and cannot but still exist. Although it is impossible to enter into details here, still it may be observed that "no atmosphere" implies practically the absence of those elements from the moon's

that we have no evidence against the existence of an atmosphere except of little importance and purely negative; on the other hand we have certain phenomena explainable by the existence of such an atmosphere, and perhaps alone by that; while we, finally, see portions of the lunar surface apparently in a condition which could not occur without one.

We may, then, legitimately assume the existence of an atmosphere as being at the very least the most probable; and it then becomes important to examine as to what conclusions we may arrive at concerning its nature and limits. Once grant its existence, and there is absolutely no reason against giving it a similar constitution to our own, excepting the vapour of water, and probably with a very much greater proportion of carbonic anhydride. With regard to its extent, it is perhaps impossible to draw at present any positive conclusions; but a remark on one point may be made. Were we to assume that the weight of the lunar atmosphere bore the same proportion to the lunar mass as that of the earth's atmosphere does to the earth's mass, it would lie very close to the surface, no portion capable of exerting a refractive power equal to one sixtieth of that of the earth extending further than two miles from the surface; but in all probability it would bear a much smaller proportion; and in either case, considering the rough nature of the limb, it could hardly, even if it extended at all, reach one second of arc beyond the limb. And as it is impossible to say what condensation and diminution would occur at the dark limb from the cold, but little expectation could be entertained of detecting it by means of the refraction of a star at the bright limb, owing to the existence of an atmosphere of practically very much less bulk, and with, at the highest, from one fiftieth to one sixtieth of the refractive power of our own.

[III.] ON THE DETERMINATION OF LUNAR TINTS.

By HENRY PRATT, F.R.A.S.

In the Report of the British Association for 1869, 'Transactions of Sections,' p. 15, is a paper on "Secular Variations of Lunar Tints," by W. R. Birt, F.R.A.S., in which this sentence occurs:—"One of the most promising lines of research having reference to the physical aspect of the moon's surface, consists in an examination from time to time of the tints which characterize every portion of the visible disk." And interesting as this branch of lunar study assuredly is, it is certainly not a little difficult to render it as perfectly satisfactory in its results as one could desire, unless some simple and thoroughly effective method be adopted both generally and systematically by lunar observers. From the time of Schröter estimations of the lunar tints have been made and referred to scales; and numerous observations have accordingly accumulated. But the scales of Schröter, Lohrmann, Beer and Mädler, and others are not directly comparable one with another, although their differences could be easily reduced to any standard scale if such were generally adopted; and although that would be useful work in relation to the labours of the selenographers just mentioned, yet it does seem in some measure undesirable, in connexion with future observations, to continue the mode of estimating tints according to *different* numerical scales. Indeed all scales are open to the objection of being to some extent arbitrary in their character; for the reference of the tint of any lunar object to a particular value, according to the scale adopted, depends very much upon the judgment of the observer, and is not the result of any exact measure or direct comparison. And this remark applies with equal force if certain lunar objects are chosen as standard points in the scale; for then the standards themselves are subject to variation in tint, and the determination of other objects by this means becomes subject to additional inexactness.

Another mode of conducting these investigations is by the direct comparison of the objects with a series of tinted disks, as in the *Homochromoscope* invented by Mr. Birt, for a description of which see the end of his Monogram of the 'Mare Serenitatis,' 2nd ed., and 'Monthly Notices R. A. S.,' vol. xxii. p. 11. One of the difficulties of this mode appears to be the production of duplicate copies of precisely the same tint; but this is not insurmountable. It would also be necessary to employ a standard illumination at a stated distance, together with one or two further details, all easily to be effected. If this instrument could be purchased as an accessory to the telescope, and it were to receive the imprimatur and recommendation of such a body as the British Association or the Royal Society, it would most likely be generally adopted by lunar observers as a standard of reference, and the work of different individuals would thus become directly comparable without the trouble of having first to extricate them from the confusion of different scales. Thus the *Homochromoscope* would become to observers of lunar tints what Admiral Smyth's chromatic scale is to the inquiry into the colours of double stars, until some as yet unthought-of development of practical optics shall produce a means of photometric analysis similar in its advance to the spectroscope. Unfortunately for lunar observers, Mr. Birt's invention has not been brought forward or developed as it deserves to be.

In the mean time there is another mode of investigation which may prove to be the best for the purpose, and but little open to objection: it is recorded in that noble work of Sir John Herschel's,

direction in determining the various tints of the localities under his scrutiny. Although the results of this mode are not absolute, but relative, yet perhaps they are the best which at present can be obtained. And there are distinct advantages to be claimed for it. No expensive or specially contrived apparatus is needed, as in any absolute determination of actual tint, all that is requisite being a good telescope and a sensitive eye. In regard to the telescope, perhaps nothing is more suited to this purpose than one of With's incomparable specula in conjunction with Browning's achromatic eyepieces. Another advantage of this method over that of affixing any arbitrary numerical scale-value to a tint is, that at the moment of observation but one idea has to be kept steadily in the mind, viz. the relative intensity of two spots; and therefore, as the mental operation at the moment is simplified, the concentrated judgment is more likely to be correct in determining the simple precedence between the two. Those who are unaccustomed to similar work may think this advantage unworthy of mention; but in practice it requires considerable care in fixing the order of two or three spots scarcely differing in tint.

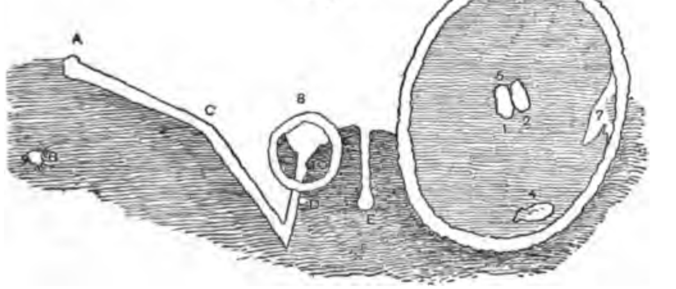
In order to ascertain the variations of the tints of any locality, it is necessary first to sketch out the immediate neighbourhood of the spot, and divide it into portions according to differences of tint,

vation of the whole region is completed. The sequence of the designations thus obtained will represent the sequence of the tints at the time of observation. Now, if the region under study be divided into ten or twenty portions, as the memory is not charged with the precise order of the letters at any epoch of observation, obviously the method is free from the effect of bias in that way, and is capable of yielding excellent results. That change of some kind has occurred in the tints of the selected locality will be evident on the accumulation of even a few nights' careful work should the sequences vary amongst themselves. It then only remains to discuss the observations according to solar altitude &c., in order to bring out the peculiarities of the variations. We may perhaps hope some day to have a means of performing actual measures of tints and reading off the resulting values as on a micrometer-head or a spectroscope-scale; but for the present such instrumental means are wanting. In the mean time it is believed that, by employing this method, which is capable of yielding the best results attainable with present appliances, and in the more general application of it by the now numerous and constantly increasing band of zealous observers, we may look for new and interesting lights upon some of the hitherto unsolved questions relating to the past and present history of the moon.

[IV.] MÄDLER (*Schmidt*), Theophilus A (*B. and M.*), IV $B^{\lambda 8}$ (*Birt*).

On the 7th of April, 1873, $7^h\ 20^m$ to $7^h\ 45^m$, Mr. Webb, with his 9·38-inch silvered glass reflector, power 212, examined this crater and its surroundings. The objects mentioned in his record are a bright point A at the extremity of a light streak, and a smaller bright point B north or N.N.W. of it. The bright streak, which was about as broad as the ring of Mädler, separated two regions, a

Fig. 1.



lighter one south of it from a darker one north of it. This streak, C, met another, D, which ran southerly through Mädler. On the dark ground between Mädler and Theophilus Mr. Webb noticed a small bright speck, E, from which a short bright streak ran southerly. The accompanying sketch exhibits the positions of these objects.

The previous history of Mädler is recorded in 'Scientific Opinion,' No. 81, May 18, 1870, p. 449. Lohrmann, who marks the crater e in Section II., gives a ridge adjoining the N.E. border, but no central mountain. Beer and Mädler, at a later epoch, give a ridge *terminating* at the north border of the crater; and an interior mountain excentrically situated towards the south border. In Rutherfurd's photogram, March 6, 1865, a ridge is seen within the crater continued from the exterior ridge, as given by Beer and Mädler; the termination of this ridge appears to be concealed by the bright illumination of the interior of the south border. On the evening of May 6, 1870, $8^h\ 0^m$ to $8^h\ 20^m$ G.M.T., the editor made the following record in his Observing-book:—" IV $B^{\lambda 8}$ west of Theophilus is a remarkable crater; a ridge, IV $B^{\lambda 10}$, in continuation of a 'crater row' north-east of Isidorus, has penetrated into its interior nearly to its south-east border. This ridge appears to have been produced subsequently to the formation of the crater, having pushed up its north border."

We trust further attention will be given to this region, especially to the bright streaks figured by Mr. Webb.

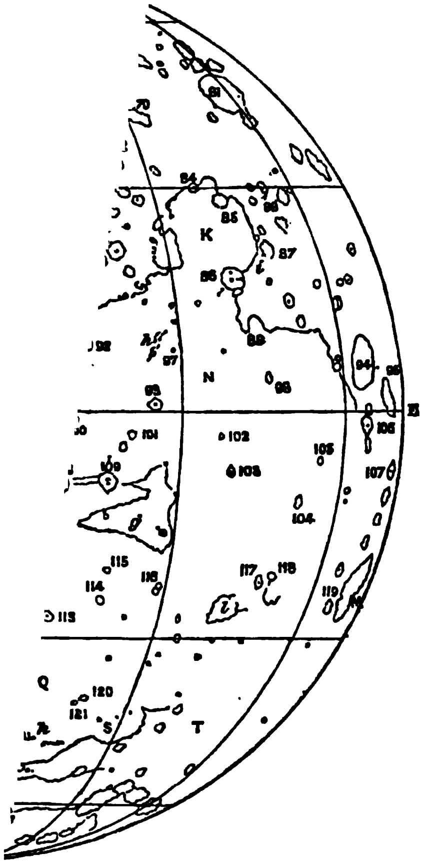
Notes.

Mr. Webb's figure is exceedingly well shown on the second edition of B. & M.'s map, the bright point A (IV $B^{\mu 1}$) being very conspicuous. The streak C (IV $B^{\mu 2}$) is marked ζ. B. & M., while showing the streak D (IV $B^{\mu 4}$) as attached to the crater *Mädler*, separate it from C. In this respect Mr. Webb's sketch indicates an addition to our knowledge in the connexion between C and D at the angular point depicted. Webb's bright points B and E are not on B. & M.; the bright streak from E is also wanting on the map. Mr. Webb appears to have missed the " crater row " from the north end of *Isidorus*. The numerals on fig. 1 represent the following objects; they are all preceded by the designation IV B^{λ}:—

1. The N.W. central peak of the mountain in *Theophilus*.
2. The S.E. central peak of the same.
5. The cleft or valley between the peaks.
4. A crater or depression within the N.E. ring of *Theophilus* (B of B. & M.).
7. A ravine within the E. border of *Theophilus*.

The streak C, with the bright spot A, which in Lohrmann's map appears as a craterlet, and the sharp angle at the conjunction of C and D, appear quite distinctly on De La Rue's photogram of February 22, 1858, and Gill's photogram of February 19, 1869, the streak D passing close by the N.E. border, as shown by Lohrmann. Fig. 1, by Webb, is in close agreement with Rutherford's photogram of March 6, 1865.

…ure Hour.')



…, the latest edition, with Additions.)

…ES.

…rey Plains.

… The Sinus Medii.	S The Sinus Iridum.
I The Sinus Aestuum.	T The Sinus Roris.
… The Oceanus Procellarum.	V The Mare Smythii.
… The Mare Imbrium.	W The Mare Australe.
R The Mare Frigoris.	X The Lacus Somniorum.

… Ranges.

g The Carpathians.	*k**The Teneriffe Mountains.
h The Riphæan Mountains.	*l**The Harbingers.
*i** The Percy Mountains.	*m* The Hercynians.

…ts, Craters, etc.

6 Manilius.	73 Arzachel.	90 Gueriké.	107 Olbers.
7*Alexander.	74 Alphonsus.	91 Parry.	108 Stadius.
58 Eudoxus.	75 Ptolemæus.	92 Bonpland.	109 Copernicus.
59 Aristoteles.	76 Triesnecker.	93 Landsberg.	110 Eratosthenes.
60 Autolycus.	77 Archimedes.	94 Grimaldi.	111 } *Beer and Mädler.
61 Aristillus.	78 Plato.	95 Riccioli.	112 }
62 Cassini.	79*Birmingham.	96 Lalande.	113 Timocharis.
63*Bond.	80 Schiller.	97 Euclides.	114 Lambert.
64*Terra Photogra-	81 Schickard.	98 Flamsteed.	115 Pytheas.
65 Clavius. [phica.	82 Capuanus.	99 Schröter.	116 Euler.
66 Maginus.	83 Bullialdus.	100 Gambart.	117 Aristarchus.
67 Tycho.	84 Vitello.	101 Reinhold.	118 Herodotus.
68 Wilhelm I.	85 Doppelmayer.	102 Encke.	119* Otto Struve.
69 Longomontanus.	86 Gassendi.	103 Kepler.	120 Helicon.
70 Hainzel.	87 Mersenius.	104 Marius.	121 Le Verrier.
71 Hell.	88 Vieta.	105 Reiner.	122 Messala.
…ar. 72 Thebit.	89 Letronne.	106 Hevél.	123 Posidonius.

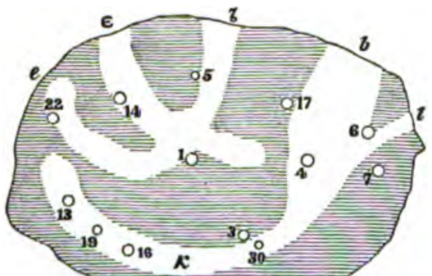
…n account of the excessive foreshortening, the scale being too small for the purpose.
…ed (*) have been added since 1862.

... the very strong probability that *activity* of a ... to render its effects visible—to such observers as ... take the trouble (not occasionally, but) with untiring assiduity to notice the appearances of the floor and faithfully record them—has been manifested between April 1869 and April 1871. The observers to whom this result is due were, during the period of the observations, entirely independent of each other; they were not even in correspondence, but forwarded their observations to the selenographer charged with the discussion. Their testimony is therefore of the strongest character; it would have been weak had but a *few occasional observations* been compared, or had but one observer have given his attention to the inquiry. As it is, the astronomical public have in the two Reports published in the annual volumes of the British Association for 1871 and 1872 the whole matter before them, from which it will be seen that the evidence has been most carefully sifted and examined; and we have no doubt that an unbiased perusal will contribute in no small degree to the formation and advancement of a truly scientific method in the study of selenography.

Two full years have elapsed since these observations were made; a few have been received since; but two circumstances conspire to retard the further prosecution of the subject—viz. the labour attendant on the discussion itself, and the absence of a recommendation by the late Committee appointed by the British Association for a renewal of the grant. While speaking of the interesting and important change which had been fairly shown—the darkening of the floor with an increase of the sun's altitude—the Report is silent on any further steps that may be taken relative to "this important branch of astronomical inquiry," except that the Committee "trusts that in future years the Association will not overlook it."

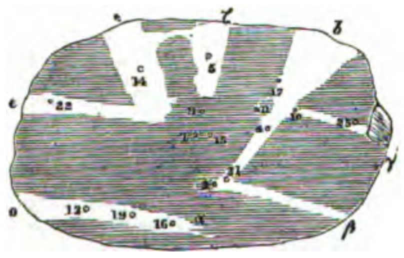
The result to which allusion has been made is, that a series of changes has occurred on the north-west part of the floor which requires for its explanation something more than the mere variations of illuminating and reflecting angles. Near the north-west border of *Plato* are three spots known by the numbers 16, 19, and 13 (see tinted plate in 'Student,' April 1870, p. 161, and plan of *Plato*, British Association Report, 1872, p. 247). These spots are ordinarily situated on a somewhat broad light streak, as in fig. 3, which is a representation of the floor as seen by

Fig. 3.



Mr. Pratt on August 23, 1869. On September 25, 1869, the streak assumed the form shown by Mr. Gledhill in fig. 4, except that the streaks ζ and ε are *not connected* as in the engraving. On November 15, 1869,

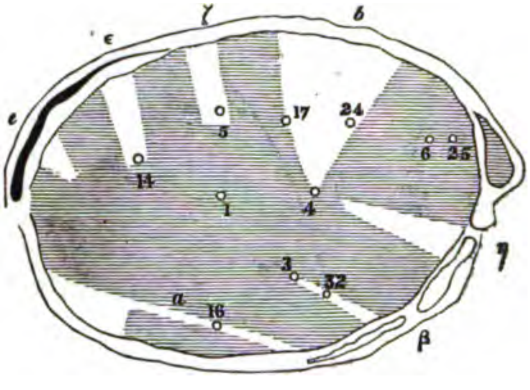
Fig. 4.



the light streak was seen by Mr. Pratt in contact with the border. On December 15, 1869, a light portion of the north-west floor was observed in contact with the border by Mr. Elger (see fig. 5). The area of this light portion was increased the following lunation; for on January 12, 1870, Mr. Elger observed it as shown in fig. 6. Again, on April 14, 1870, a change had supervened on this part of the floor; for the streaks had assumed the form shown in fig. 7. From the month of November 1869 to the month of May 1870 the whole of this part of the floor was

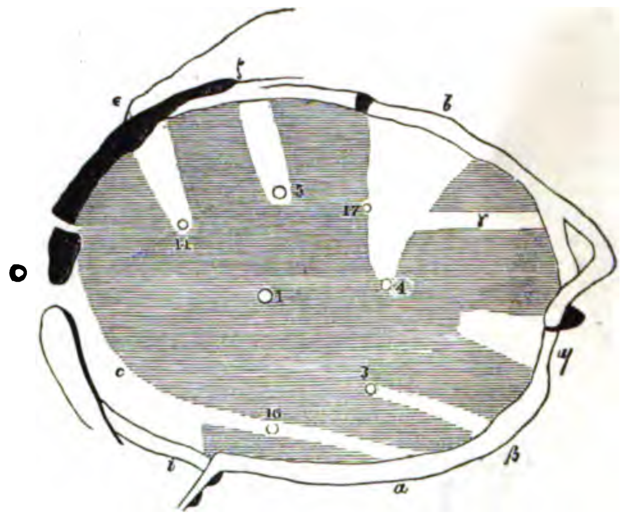
... westward of the streak a on fig. 4 (designated "o") was seen. It may be well to mention that on October 17, 1869, Mr. Gledhill aligned

Fig. 5.



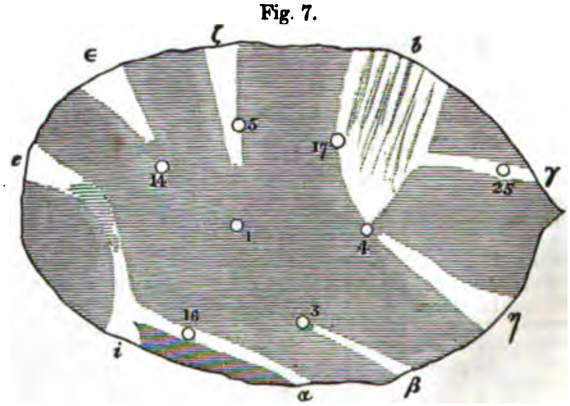
the two (seen as one streak), and found that the alignment, if produced to E.N.E., would cut the north border of the crater 438 (Mrs. Jackson Gwilt). *Webb, third edit.* The history of the western streak "*o*" is

Fig. 6.



curious. It appears to have been lost after the subsidence of the brightness on the north-west part of the floor. Its latest appearance in the course of the luni-solar day was in interval 72 to 60 hours before sunset

Fig. 7.



on September 25, 1869. It was not seen after the sun's meridian passage at *Plato* later than November 19, 1869. The last distinct mention of "*o*" appears to have been on April 11, 1870, when it was recorded as very hazy and ill-defined. In May and June it appears to have merged into the general brightness of the floor. In July a condensed brightness

1868 and August 1870, the stream ... might have ... of ejecta towards the west border. In December 1869, January, March, April, and May 1870, the eruption might be supposed to have proceeded with greater energy, a flowing of ejecta occurring about the 14th of April in the old channel, *c*; the ejecta in May and June overspreading the whole of the north-west part of the floor and producing the brightness witnessed. As this brightness subsided, the new condition of the flowing of ejecta towards the arm of the trident *e* along the channel *c* became permanent in August and September 1870 (see figs. 6 and 7).

Fig. 8.

Mr. J. W. Durrad has furnished a drawing of *Plato*, 1872, Dec. 8, which is reproduced (fig. 8) for the purpose of directing attention to a crater (*a*) in the N.W. gap of the border. This crater has not, so far as our knowledge extends, been previously observed; and it is very desirable that it should be confirmed or otherwise.

We invite criticism on this communication, with the especial object of showing that these changes are explicable on the principles of variation of illumination and libration; and we shall be glad to receive records of observations of the appearances of the north-west part of the floor for comparison with the above and other drawings in our possession.

Mr. Neison has furnished the following remarks on Mr. Proctor's suggestions relative to the darkening of the floor of *Plato*:—

"The hypothesis so admirably elaborated by Mr. Proctor as an explanation of the darkening of the floor of *Plato* as the solar altitude increases ('Quarterly Journal of Science,' January 1873, pp. 50–53) is open apparently to very grave objection. Mr. Proctor asks, are we not to regard this change as due to physiological causes? and 'whether, in fact, the neighbourhood of the dark portion ... *Plato*, we find, as the sun's altitude increases, at first gradually lightens until (occasionally) it becomes nearly as light as the *Mare Imbrium*, and then rapidly darkens to an iron grey; *Archimedes*, however, while likewise rapidly lightening at first, remains unchanged of a pure greyish yellow. How is this difference to be explained if we find the change in *Plato* dependent upon a physiological effect? *Archimedes* as well as *Plato* is in the vicinity of the terminator. Even if we take a spot as dark as *Plato*, say *Boscovich*, we find this spot retaining its dark colour practically unchanged throughout the lunation; and a similar comparative fixity of tint is observed in the *Mare Imbrium* between *Plato* and *Archimedes*. This difference is surely sufficient to show the difficulty of ascribing the change of tint in *Plato* to physiological effect or contrast; for, be it remembered, the wall of *Archimedes* is even brighter than that of *Plato*, while the dark colour of the latter is fully as well marked in photographs as to the eye, showing it has not its origin entirely in that organ, seeing that *Plato* is occasionally nearly as light in tint as the *Mare Imbrium*. Dr. De La Rue's results may be made to bear out apparently, to some extent, Mr. Proctor's hypothesis; but it is perhaps open to question how much reliance may be placed on them for this especial purpose.

"It is to be regretted that a more rigorous and accurate test than mere eye estimations could not be used, as errors may easily be introduced in combining observations made by different observers; nevertheless with a sufficient number of observations, each observer's being treated separately as well as combined, fairly accurate results are easily obtainable. Any such method as that suggested by Mr. Proctor would undeniably be of assistance; but it is not suited for the generality of observers. Not only does it require a perfectly mounted instrument, with special adjustments, but there are no objects on the moon's surface that are, including *Plato*, entirely uniform. *Plato*, in fact, is covered by numerous very faint, light grey streaks, very constant in tint it is true, but which would render any contrivance to remove the effects of contrast inoperative."

[In reference to Mr. Proctor's remark on the attainment of the greatest diurnal heat about two hours after noon (p. 52), the Rev. F. W. Stow shows ('Quarterly Journal of the Meteorological Society,' April 1873, p. 138) that the maximum in the sun occurs *soon* after noon.—Ed.]

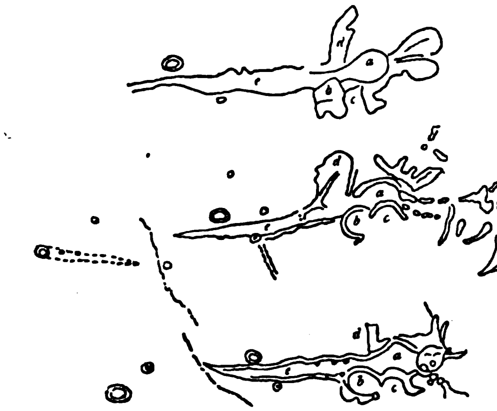
[VI.] THE WEDGE-SHAPED VALLEY OF THE ALPS.

This very interesting object has commanded much attention from observers, but we are not aware that it has been very closely studied, as, for instance, in the manner in which *Plato* has been. Drawn originally by Bianchini, scrutinized and delineated by Schröter, and also by Lohrmann and Beer and Mädler, it has become one of those objects that is sure to be pointed out to observatory visitors as exceedingly remarkable; but as for its correct delineation, and especially for an enumeration of its principal features, we have been, and as regards the last-mentioned still are, deficient. It was on January 25, 1866, that Webb surveyed it with his 7-ft. telescope, power 212, the eyepiece being formed of two achromatic microscope object-glasses; and on that occasion he was able to obtain a drawing of it, which is preserved side by side with Schröter's, Lohrmann's, and B. & M.'s in the 'Intellectual Observer,' vol. ix. (April 1866), p. 175, and which he described in the following words:—

"The appendages branching out from the east end of the valley in various directions will be understood to be mountains, a portion of the Lunar *Alps*. The whole interior of the valley from end to end was so perfectly defined by its pale grey tint and its peculiar and almost unbroken evenness, as to admit of no doubt as to its real termination, which is carried considerably further to the east than is shown in the design of B. & M. In this part, as I saw it, it took the form of a pear or Florence flask, whose longest diameter made a very obtuse angle to the south with the general direction of the valley. This portion, which must in reality form a noble amphitheatre, is evidently much better represented by Schröter than by either of his successors. Its length might amount to about one sixth of the whole. The *neck*, so to speak, of the flask, of about equal length, was a part where it was throttled, especially on the north, by encroaching mountains, till its width was only one third, more or less, of that of the broadest part west of it. A low ridge running lengthways was here visible in the bottom connected with the south side, and having somewhat the character of a landslip from the cliffs in that direction, which rose ruggedly and irregularly above it. At some distance further west a similar style of cliff occurred again; but there was no appearance of a landslip there. With these exceptions, the south side was pretty regular and not far from straight. The opposite side, on the contrary, was considerably indented, giving various widths to the bottom in different places; the greatest width, somewhat exceeding that of the flask, lay a little east of the centre. One of Schröter's two little craters was plainly visible; but the other, lying on the interior declivity, could not be traced; a slight bending towards the north could be perceived at the narrow west extremity of the valley, but not to the extent indicated by Lohrmann. Beyond the east end a very narrow irregular gorge continued the line of the south edge through a lofty part of the Alps, widening out at length into the *Mare Imbrium*."

We have reproduced Mr. Webb's drawing for the purpose of comparing it with two subsequent drawings of considerable excellence by M. Gaudibert and Mr. Elger. M. Gaudibert's was made on May 28, 1871, and Mr. Elger's on December 11, 1872. M. Gaudibert says:—

Figs. 9, 10, 11.



"While looking attentively at this valley I was not a little surprised to find three markings resembling three craterlets. Of those near the mouth of the valley I am pretty certain; but the third may be the shadow of a projecting peak."

Mr. Elger's remarks are as follows:—

"While gazing at this remarkable formation at lunar sunrise, one can scarcely fail to be impressed with its thoroughly *earth-like* character. Unlike most of the objects which come under the notice of the student of the moon's surface, the 'wedge-shaped valley' has its counterpart in many mountainous regions of our globe: for example, the River Laagen in Norway flows for nearly sixty miles through a comparatively narrow gorge which presents features very similar; but perhaps its most striking analogue is the higher valley of the Arkansas in Colorado: this valley is upwards of sixty-five miles in length, it is five miles in width at its upper end, and gradually narrows to about two miles at its lower end; its northern extremity, where the river has its source, bears a close resemblance to Webb's 'noble amphitheatre.'"

e. The main portion of the valley.

C. E. Burton, Esq., writes from Loughlinstown as follows:—

"April 5, 1873. L.M.T. $8^{h}\ 50^{m}$ to $9^{h}\ 20^{m}$. 8-inch silvered glass reflector. Terminator between *straight wall*, and the small crater next to it on the east and bisecting *Plato*. About two thirds of the length of the valley is traversed

pears at the summit of the south bank, which is scarcely broken down, and after running some miles southward is lost among hillocks. [There are some indications of this cleft in Mr. Elger's drawing.] A deep hollow like a crater is plainly visible with 400 on the 8-inch reflector, where the rill meets the floor of the valley similar to that on the west side of *Hyginus*. Three shallow canals, two of which are somewhat indistinctly marked, run southwards from points near to the west end of the valley for fifty or sixty miles."

[VII.] COPERNICUS,

March 8, 1873, from 6^{h} to 10^{h} P.M.

This evening the air was clear and steady as a whole. I observed the crater Copernicus. On account of so many craterlets situated in its immediate neighbourhood on the west, I tried various powers up to about 800 in order to see whether there were any of that kind either on its ramparts, slopes, or floor; but none were visible. Though this last power showed many more details than those given in the accompanying sketch, yet I could not use it with best results on account of imperfection in the state of the atmosphere. The sketch was made with powers 120 and 200. Three mountains, marked 1, 2, and 3, attracted my eye first when looking towards the centre of the floor; but with closer attention I found that mountain 1 is formed of three, closely packed together — one on the north marked 4, and just separated by a black line, another (*b*) on the south and extending towards the east, and the third (*a*) extending towards the south-west. These two (*b*, *a*) may be only two distinct peaks of mountain 1. Mountain 2 has a smaller one close on the south (shown in sketch, fig. 12). Its north side has a kind of semicircular cavity, like the half of a craterlet (also shown in sketch), casting at this time a very short shadow. Mountain 3 seems single and quite isolated. Two minute hillocks are situated just south of 3, between 2 and *a*. If all these objects were included in the expression "central mountain," I believe nine distinct peaks could be counted there; but I see Mr. Webb ('Celestial Objects,' third edit. p. 97) restricts them to six. The other objects, both on the floor and along the interior slope of the ramparts, visible at this time, need no particular description, with three exceptions; for they are all hillocks of various forms and sizes. The exceptions are 19* and 20* on the floor. These objects are more difficult to see than perhaps any others shown, on account of their being very low and their contours very undefined. The third exception is 9* on the eastern interior slope; this hillock, though very small, was very bright. The vacant space between the central mountain down to the north extremity of the floor is not perfectly even; but power 800 showed a large amount of ruggedness which I found impossible to delineate. The objects on the floor are numbered separately from those along the slope of the ramparts.

Fig. 12.

C. Gaudibert.

N.B.—9, *n*, and *m* are represented by B. & M. as a ridge; and so it looks with power about 100.

[On the 6th of May, 1873, between 7.30 and 9.10 G.M.T., I examined M. Gaudibert's drawing at the telescope, aperture 2·75 inch, power 100, definition very fine. To avoid misconception, it may be well to remark that on the interior eastern slope the objects figured by M. Gaudibert *are not isolated, but are the highest points of the interior ridges.* Nos. 10 and 11 were made out easily; also No. 9, which was not particularly remarkable for brightness. The following alignments were taken:—Central mountain, No. 1, longitudinally and through its shadow cuts the middle of the lacuna *south* of the peak on the west summit, which casts the pointed shadow; a line carried through the two principal mountains 1 and 2, cuts 10 and the northern part of 11 on the east, and a bright depression *north* of the peak A, the same which casts the pointed shadow on the west.—Ed.]

[VIII.] MARIUS,

January 11, 1873. 8^{h} to 10^{h}; power 100.

Fig. 13.

Weather unfavourable for the delicate markings of this crater, the air being "thick" and windy. Best power 100.

The floor of this crater is not level, but slightly raised like a wide band in the middle from one end of the floor to the other, from the N.W. to the S.E. Upon this slightly elevated region are situated the very white (perhaps on account of contrast) craterlet 1, itself at the top of another slight eminence, also the mound 2. These two objects, the only ones yet observed, as far as I know, are mentioned by Mr. Webb. That portion of the floor (3) situated N.E. of the middle band was visibly much darker than any other*, and seemed depressed. The dark tint went up the declivity towards 1 and 2, leaving a white space between. The opposite side of the floor was of a light grey. 4 is a doubtful white spot.

C. Gaudibert.

* This darker band is not shown in the engraving, but is strongly marked in the original sketch.

Mr. J. W. Durrad observed Marius on April 10, 1873, aperture 3·625; he saw the craterlet 1, and the doubtful white spot 4. The mound 2, as well as the darker N.E. part of the floor, escaped him.

[IX.] RECENT OBSERVATIONS.

M. Gaudibert has communicated to the 'English Mechanic,' April 25, 1873, p. 143, an outline sketch of *Pitatus* and *Hesiodus* with angles of position and fifty-six lettered and numbered objects, as observed on April 6, 1873, with his 6-inch silvered glass reflector. The following remarks are important:—"On the south-east rampart are two craters (11 and 15) joined by a valley (12), the sides of which are very much broken. B. & M. give these two craters; but instead of the broken valley they have here three other craters." M. Gaudibert suggests that the three craters may have crumbled down since B. & M.'s time.

"46 is a central craterlet on the floor of *Hesiodus*. B. & M. shade the extremities of this floor all around, and leave the centre perfectly white, as if the floor were raised there; but they give no craterlet. Had the craterlet existed in their time, could they have missed it? It is a very easy object now."